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Institution of electrical engineers

JOURNAL

OF THE

SOCIETY OF TELEGRAPH-ENGINEERS AND ELECTRICIANS.

FOUNDED 1871. INCORPORATED 1883.

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

PROF. W. E. AYRTON, F.R.S., CHAIRMAN.

VOL. XIII.—1884.

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ERRATA.

In the lithograph facing page 312 in No. 53 of the Journal, the figures to the left-hand side of Fig. A should be 0, 5, 10, 15, 20, 25, 30, 35, 40.

For 58° F., line 19, p. 372, No. 53, read 78° F.

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VOL. XIII.

JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XIII. 1884. No. 51.

The One Hundred and Twenty-eighth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 10th, 1884—Mr. WILLOUGHBY SMITH, President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates were announced and suspended.

The following transfers were announced as having been made with the approval of the Council:—

From the class of Associates to that of Members—

Shelford Bidwell, M.A., LL.B.
William H. Massey.

From the class of Students to that of Associates—

A. E. Kennelly.

Donations to the Library of the Society were announced as having been received from the Cambridge Philosophical Society, the Ohio Mechanics' Institute, Professor G. Édard, the Commander E. D'Amico, Local Honorary Secretary for Italy; J. E. H. Gordon, B.A., Member; Dr. C. Lemon, Member; John Munro, Associate.

On the motion of the President, the thanks of the meeting were cordially given to the donors.

The PRESIDENT stated that a Committee had been appointed to consider the receipt of the book recently presented to the

Society by Mr. Davy. It was reported to be a very valuable book, and he regretted that, not having seen it, he was unable to say more about it; but the members would call to mind how prominently the works of Mr. Davy had been brought before the telegraph world by Mr. Fahie, to whom great praise was due.

The PRESIDENT, continuing, said: When I first occupied this chair as your President, I expressed a wish that the Members of Council would extend to me the kind assistance they had given to my predecessors; and now that the time has come for me to vacate this chair, I think it would be ingratitude on my part were I not most heartily to thank those gentlemen, collectively and individually, for the kindness and assistance I have received at their hands during my year of office, as also Mr. Webb, our worthy Secretary.

Taking the interest I do in this Society, it is very gratifying to me to know that I resign this chair to so able a gentleman as the one you have elected as President. There is one more task for me to do, and that of a very agreeable character. I have very great pleasure in presenting to Mr. John Munro a cheque for £10, being the premium awarded by the Council for his able paper on "New Telephone Transmitters," which he read last year.

The cheque was then handed to Mr. Munro by the President, amid the applause of the meeting.

The PRESIDENT: Now, gentlemen, nothing more remains for me to do except to ask Professor W. G. Adams to occupy this chair and commence his duties as your President.

Professor W. G. Adams, President-elect, then took the chair.

Mr. W. H. PREECE: Mr. President and gentlemen,—Before our new President commences his duties for the ensuing year, I beg to propose that we accord our thanks to Mr. Willoughby Smith for the very able way in which he has discharged the functions of President during the past year. I can speak from some experience on this question, having served the post myself; and I know full well how arduous and constant are the duties required of the President of the Society to keep things going with that *éclat* that has sustained this Society for so many years. But I am bound to confess that however hard I worked, and how-

ever successful my predecessors and successors may have been, we all must lower our flags to the assiduity and attention which Mr. Willoughby Smith has displayed during the past year. I do not think he has missed a single meeting. He has added to our pleasure and instruction by himself bringing forward papers of very prime importance. I propose a vote of thanks to Mr. Willoughby Smith for all he has done for us during the past year; and, in doing so, I also will express the hope that our incoming President may have as successful a year of office as that which Mr. Willoughby Smith has just passed through.

Mr. Crampton: I beg to second the proposal, and in so doing may state that, when the President mentions the assistance given him by the Council, he overrates that assistance. Although we were ever ready to relieve him as much as possible, we could do but little, as he, with his well-known energy, looked into everything himself, and prepared with the Secretary all matters so carefully that our duties became light indeed; therefore, in seconding the proposal, I do so with great pleasure, and wish him a happy new year.

The President: Gentlemen,—My first duty in this chair is a very pleasant one. You have heard the proposal made by Mr. Preece, and seconded by Mr. Crampton, and I ask you to cordially give your thanks to our late President for the able way in which he has carried on the business of the Society during the past year.

The vote was cordially expressed.

Mr. Willoughby Smith: It is a maxim of mine, that anything worth doing is worth doing well, and, to the best of my ability, I have always done things that way. I am extremely obliged to Mr. Preece for his kind remarks, also to Mr. Crampton for the way in which he has spoken of me; and I am thankful to the meeting for the very cordial manner in which those remarks have been received. It is most gratifying to me to know that my humble endeavours have been crowned with success.

The President then read his Inaugural Address.

INAUGURAL ADDRESS.

Delivered by Professor W. Grylls Adams, F.R.S.

I will not occupy your time in speaking of myself or of my own feelings, nor will I attempt to discover on what grounds you have conferred on me the great honour of selecting me as your President; but I trust that our Society will advance no less rapidly than it has done in the past, and that it will continue to lead the way in electrical science.

I propose to draw your attention this evening to the rapid growth of electrical science, and to those principles of the science of energy of which dynamic electricity now forms a part. In tracing the history of the science of electricity, one cannot but be struck with the number of distinguished men who within the period of one hundred years have devoted themselves to its development.

About the middle of the last century, the discovery of the first accumulator of electricity by Cuneus, and the experiments and theory of Franklin, drew the attention of the scientific world to the phenomena of electricity, and induced men like Æpinus and Cavendish, and after them the Earl of Stanhope and Coulomb, to apply their powerful intellects to the working out of the principles of statical electricity. They were men who were capable of treating the subject both experimentally and mathematically—*i.e.*, of first making sure of their facts by testing them experimentally, with apparatus often of their own construction, and then of working out by mathematical treatment the full consequences of those facts, and gathering them up into the laws of electrical action. From that most interesting volume edited by the late Professor J. Clerk Maxwell, "The Electrical Researches of the Hon. Henry Cavendish," we are able to see exactly the point to which the science of electricity had been pushed by him just one hundred years ago, and we find that he was a man who had stepped far beyond the men of his time, and whose work remained quite unknown to those who came after him.

The idea of electric potential was clearly present to the mind of Cavendish: he defines what he calls the "degree of electrifica-

tion" as the compression—*i.e.*, *pressure*—of the electric fluid. He compares the ratios of the charges of bodies to that of a sphere, and speaks of a body as capable of holding so many "inches of electricity"—*i.e.*, a body whose capacity is equal to that of a sphere whose diameter is that number of inches. In fact Cavendish took a sphere of *diameter* 1 for his unit of capacity, whereas we now take a sphere for our unit whose *radius* is unity.

In estimating capacities he employed the method which recently Weber and Kohlrausch have employed to determine the ratio of electrical units. He proved that there is no free electricity in the interior of a closed conductor (the globe and two hemispheres), and proves from this that the electric force varies inversely as the square of the distance. He also compared and measured the numerical value of the specific inductive capacities of different substances.

In the absence of galvanometers, Cavendish compared the resistances of iron wire and salt water by making his body and the iron wire the branches of a divided circuit, and discharging a condenser through them; he then replaced the iron wire by such a length of salt water as would make the shock appear to be of the same strength. From this he concluded, rightly, that the resistances of the iron wire and salt water were equal. The accuracy of Cavendish's results was remarkable, for his results agree with the resistances of these substances found by our present methods at a temperature of 11° C. Taking himself for the galvanometer, he employed the method adopted by Professor Barker and Mr. Crookes, at the Paris Exhibition, to determine the electro-motive forces in their tests of incandescent lamps. As they knew the value of the shunts, they could determine their electro-motive forces, whereas he employed the method to get the value of his shunt, or rather to find when two shunts were equal.

Cavendish's electrometer consisted either of a pair of straws with cork balls on their ends, or a pair of pith balls suspended by silk strings from a glass rod, and yet with these he was able to compare the capacities of insulated conductors. His experimental determinations of the capacities of long narrow cylinders have

been compared by J. Clerk Maxwell with their capacities as calculated by himself, with the following results:—

	Cavendish.		Maxwell.
Capacities...	5·669	...	5·668
"	5·754	...	5·775
"	6·044	...	5·907

While Cavendish was working in England, Coulomb, in France, was investigating, by means of his delicate torsion electrometer, the laws of electrical attraction and repulsion; he also investigated, both experimentally and mathematically, the laws of distribution of electricity and of loss of charge from conductors, and his results were given to the world about 1785, after Cavendish's work was done. Coulomb's torsion balance was almost the only electrometer for accurate determinations until the electrometers of our own time, invented by Sir Wm. Thomson. After the times of Cavendish and Coulomb there was a lull in the pursuit of electricity, and the attention of the scientific world was drawn more especially towards the study of chemistry, which advanced rapidly toward the end of the last century, until in the year 1800 the uniting of these two sciences by Volta gave rise to the further development of electrical science in the form of voltaic electricity, and led the way to the grand discoveries of Davy, and of Faraday, and of Daniell on the relation of chemical action to electric currents.

I cannot stay to dwell on the early days of magnetism,—the days of Gilbert and of Halley,—nor need I do so, since one of my predecessors, Sir Wm. Thomson, has given us some account of the early progress of terrestrial magnetism, but would again point out that it was by the uniting of the two sciences of magnetism and of current electricity in the year 1819 or 1820 that Œrsted paved the way to the very rapid spread of electrical science, when almost each succeeding year gave birth to some new group of facts or some new instrument, or saw some new step taken which was important enough to be regarded as a new principle. Thus from 1820 we have Ampère's theory of magnets and of terrestrial magnetism, his laws of the action of currents on magnets, and on the mutual attraction and repulsion of currents; also the magnetic action of solenoids. We have also the invention of the multiplier,

or galvanometer, and improved forms of this useful instrument, and the discovery of the first principles of electro-magnetism, all crowding on one another. The eleven-year period, from 1820 to 1831, not only embraced all these discoveries, but gave us also the laws of Ohm, and the work of Faraday on the induction of electric currents and on the evolution of electricity from magnetism, laying the foundations of the subject of magneto-electricity. As regards the growth of new principles, probably no period of eleven years has seen the science advance with such rapid strides.

These principles having been established, a new era in the history of the science begins with the methods of absolute measurement adopted by Gauss in 1832, in which he showed how the intensity of the earth's magnetic force and the magnetic moment of a magnet could be measured, so taking the earth's force as the standard of comparison for the measurement of magnetic forces; and this was soon extended by Weber to electric currents.

The Science of Energy.—In looking back over the history of a science, it is important to take into account the order in which scientific discoveries have been made. Since the earlier researches of Faraday, the labours of Joule, of Clausius, of Maxwell, and of Thomson have given us a new science, and have shown us that it is probably universal in its application, and that all the physical sciences may be regarded as linked together and as forming branches of this new science—the science of energy.

Had the science of energy been developed before Faraday's investigations were made, we shall hardly be wrong in conjecturing that his master-mind would have arrived at the conclusion which has been since established by Helmholtz and by Thomson, that the phenomena and the laws of induction currents are but deductions from the applications of the laws of energy to the discoveries of Œrsted and Ampère.

In Œrsted's experiment on the motion of a magnet in the neighbourhood of a current in a conductor, the potential of the magnet with respect to the conductor is altered by its motion, and the energy communicated to the magnet depends upon the rate at which this potential changes.

This work done by the current, together with the work done

in heating the circuit, must be equal to the work spent in keeping up the current in the circuit. If no work is so spent, *i.e.*, if there is no electro-motive force, but only a conductor forming a complete circuit, then the motion of the magnet will generate a current in such a direction as to oppose the motion, whose energy will be spent in heating the conductor. Here, then, we have Faraday's induction current. Maxwell has shown us how both the induction of currents and the mechanical forces between them depend upon their coefficients of mutual induction through the medium surrounding them, and that in estimating electrical energy a portion is spent outside the conductors on the medium through which the induction takes place.

The principles of energy tell us that the mechanical energy spent in carrying the conductor across the magnetic lines of force of the earth, or of a magnet, is equivalent to the electrical energy produced within and around the conductor by the induction current.

We see here that there is some distinct and intimate, but as yet mysterious relation between the sciences of motion, of magnetism, and of electricity, and we also see that in these simple experiments are involved the principles with which we are concerned in our consideration of electric motors. The accurate measurements of Joule, of Peltier, and of Thomson have shown the definite relations which the sciences of motion, of electricity, and of magnetism bear to the science of heat. The labours of Maxwell, based on measurements of the velocity of light and of specific inductive capacities, have given us some clue to the relationship between electro-magnetism and the phenomena of radiant heat and light, which must be regarded as only different effects of the same form of energy.

In electrical science, then, as in the science of motion of masses, we have to do with the principles of energy, the first law of which, the principle of conservation of energy, tells us that *whenever work is performed by means of heat, an amount of heat disappears which is equivalent to the work performed, and whenever work is spent in producing heat, the heat produced is equivalent to the work performed.* Applying this to magneto-electricity, Joule

showed that heat may be created by working a magneto-electric machine, and that if the current is producing no other kind but thermal energy, the total quantity of heat is equivalent to the work spent in producing the current. To which Thomson has added, that when a current produced by heat is made to work an engine, the difference between the heat absorbed and the heat given out is equivalent to the mechanical work done by the engine.

The advantages of exact observation and measurement, when a science has passed through its early stages, are seen in the power it gives of making new discoveries, by determining precisely how much of the observed phenomena may be due to well-known causes, thereby showing how much still remains to be accounted for. Let me illustrate my meaning by an instance. A magnet suspended by a silk thread oscillates about its position of rest in the magnetic meridian, and is gradually brought to rest by the resistance of the air and the stiffness of the thread. But the effect of these causes being exactly known, it is found, as was first shown by M. Arago, that if a copper disc be brought near to the the magnet it will come to rest much more quickly than when the copper is absent; hence, in order to damp the oscillations of a galvanometer needle and bring it quickly to rest, it may be surrounded with a mass of copper. From the investigations of Faraday we know that currents of electricity are set up in the copper by the motion of the magnet, and that these currents react upon the magnet and check its motion.

Now let us see how the exact investigations of Joule on the principles of energy clear up our ideas on these and similar phenomena. In consequence of the earth's magnetic action upon it, the oscillating magnet has a certain amount of energy of position when it is at the extremity of its swing, which is transformed into energy of motion as it swings across the magnetic meridian in which it is suspended. Part of this energy is spent in overcoming the energy of torsion of the suspending fibre, in consequence of which the swings are diminished in extent. Another part is spent in communicating energy to the air by friction and by producing eddies in it. Another portion is spent in producing electric

currents in the copper damper. The energy so spent may be determined experimentally by vibrating the needle of a galvanometer first without, and then with the copper damper.

Let me borrow another instance from the energy of motion in a resisting medium. Before Froude made his celebrated experiments on the resistance to the motion of ships through the water, it was supposed by mathematicians that, in consequence of the perpendicular pressure of a fluid against the ship, there was work done by the ship moving through the water in removing the water out of its path. Not unfrequently the friction against the side was supposed to be so small in comparison with this that it was entirely neglected. Now Froude has shown that nearly all the energy spent in driving the ship through the water is spent in overcoming the work of friction against the sides, and the internal friction or viscosity of the water. The same is also true of the motion through gases, part of the energy in these cases being spent in producing eddies in the fluid.

In the progress of electricity the steps are clearly shown which usually mark the progress of any science. At first there is an accumulation of observations or of facts, which may appear wonderful in themselves and unconnected with one another; these may be classified and arranged under their respective heads, but they acquire a new significance when they are shown to be instances of a more general law, which binds them together, and forms a groundwork for the pursuit of further and wider investigation.

The discovery by Faraday, in 1831, of the time interval before an electro-magnet gets up to its full strength, and the experiments of our late President on induction currents, are but instances of the law that all electro-magnetic phenomena require time for their development. Maxwell in his excellent treatise on electricity and magnetism has put this matter very clearly, and has shown that the velocity of transmission of electro-magnetic influence is the same as the velocity of light on the wave theory; he has also fully shown how the different electro-magnetic phenomena are related to the principles of energy;—that, in fact, the work done by an electro-motive force acting on a conducting circuit is energy

of motion; that part of this energy is spent in producing electro-magnetic phenomena, such as the motion of conductors—for instance, the revolution of the armature of a dynamo machine used as a motor; that another part is spent in overcoming the resistance of the circuit, and that this part is converted into heat; and that the remainder is spent in increasing the energy of motion of the current, giving rise to the phenomena of induction currents.

The rapid progress in the discovery of the principles of electricity, and in its practical applications, is especially due to the fact that those who have taken the lead, both in theory and in practice, have been men who have seen the full importance of accuracy of measurement in all that pertains to physical science, and, by the instruments which they have invented, have themselves greatly contributed the means of attaining that accuracy in the measurements of electricity.

We could hardly take a better instance of the progress of the theory of electricity, and the methods of overcoming difficulties in its practical application, than the discovery by Sir William Thomson of the law of retardation in submarine cables, followed by his invention of the mirror galvanometer and his siphon recorder. Having found that this retardation is proportional to the square of the length of the cable, and that the disturbance, as it travels, spreads out into a long wave on which any number of little ripples may successively be superposed, he invents the instruments by means of which those ripples could be made to give the message through the cable at a speed which would be practically useful.

When speaking on the subject of the importance and the methods of accurate measurement in electrical science, and on its relation to the science of energy, it is right that I should draw attention to the work of the late Sir William Siemens, whose loss will be deeply felt, not only by his family and friends, but in the scientific world and in every branch of the profession of engineering. His scientific labours extended into several branches of science—perhaps I should rather say that he approached the other sciences through the science of energy. He was a practical

engineer, who, while not neglecting the teachings of experience, yet felt the full importance of founding his practice on a true scientific basis, and who has been distinguished for the way in which he has devoted himself to the storing up of wasted energy. His first visit to England took place in the year 1843—the year in which Joule firmly established the principle of conservation of energy by his experiments on the friction of water and on the heat generated by the magneto-electric machine. The statement made by Joule, that "wherever mechanical force is expended, an exact equivalent of heat is always obtained," was not readily received by the leaders in science, even after a second important paper on the subject in 1847 (when Sir William Thomson's attention was first drawn to the subject), yet in that very year Sir William Siemens was applying the principle to practice by storing up heat in his regenerative condenser; and in 1853, in his paper on "The Conversion of Heat into Mechanical Effect," he defines a perfect engine to be one in which all the heat applied to the elastic medium is used up in causing its expansion within the cylinder, leaving no heat to be thrown into a condenser or into the atmosphere. From this time his attention was directed to the principle of regeneration as applied to condensers and steam-engines, and a few years later, with his brother Frederick, he invented the regenerative gas furnace. Nothing but the conviction of the importance of preventing the waste of energy would have carried him through the first unsuccessful attempts to make steel on the open hearth of a regenerative gas furnace; but in 1867 his labours were crowned with success, when steel of good quality was produced and shown at the French Exhibition. Our ideas of the actual temperature of furnaces have become more definite since Siemens constructed his pyrometer and his electrical resistance thermometer.

It was early in the year 1867 that Sir Wm. Siemens brought before the Royal Society the results of researches, based on an idea thrown out by his brother Dr. Werner Siemens, whereby he established the principle which forms the basis of all dynamo-electric machines.

An idea had been advanced by Hjorth in 1854, that the

current from the armature of a magneto-machine might be sent round certain electro-magnets so placed as to strengthen the magnetic field of the permanent magnets, but this idea had been lost sight of, and in February, 1867, two very important papers were brought before the Royal Society on the same evening,—one by Sir Charles Wheatstone and the other by Sir Wm. Siemens,—in which the principle of the action of dynamo machines was fully set forth. We may call it the principle of the regenerative action of electric currents. On the same evening Sir C. Wheatstone pointed out the great advantages of the divided circuit arrangement, which is now adopted in shunt dynamo machines—advantages which (as shown by Sir Wm. Siemens in his recent paper before the Royal Society) greatly increase the steadiness and efficiency of the machines.

The past year has seen great advances made in the application of electrical energy to metallurgy and in the electrical transmission of power: in these branches Sir William Siemens was always found in the front rank. Time will not now admit of my considering fully the speculations as to a fan-like solar action which Sir William Siemens has recently put forward to restore and keep up solar energy. One cannot help seeing in this, as in all the work of his busy life, how difficult to him was the idea of wasted energy.

Sources of Energy.—It has been well said that solar heat is the principal source of energy available to man. This solar heat promotes evaporation and lifts the vapour to the mountain tops, whence it descends again in streams which may be regarded as vast stores of energy of motion, readily convertible into useful work by means of turbines, with a loss of only 25 per cent., *i.e.*, with an efficiency of 75 per cent.

The earth has been storing up solar energy for ages in our coal-fields. This energy, again converted into heat by the combustion of the coal in oxygen or in atmospheric air, may be converted into mechanical work, and made use of by means of heat-engines, but in every conversion of energy there must always be some loss, and especially in the conversion of heat into work, because in the form of heat, energy is most readily dissipated.

In approaching the practical applications of this science of energy, let me first draw attention to the conversion of heat into work, and to the efficiency of heat engines.

In every form of engine which is practically useful for the conversion of heat into work, we have to deal with a substance at two different temperatures, and the work is done by the expansion of the substance against the external pressure of a piston by means of which the work is accomplished—thermo-electric engines being as yet possible, but not actual sources of supply.

Thus, in a hot-air-engine, heat is applied to air enclosed in one part of a cylinder; this air expands and drives back a piston, so doing work and losing its heat. If the air is further cooled on expanding by means of a refrigerator, a greater amount of work may be done, the amount being directly proportional to the difference of temperature of the air in its hot and cooled state—*i.e.*, the work done is proportional to $(T - t)$. The efficiency of the engine is the ratio of this work to the total heat given to the air—*i.e.*, if T is temperature from absolute zero, the efficiency of the engine is $\frac{T - t}{T}$. Hence the efficiency of such an engine depends on the difference of temperature through which it works.

By the second law of energy, or thermo-dynamics, it is established that in all heat-engines the work which a given quantity, Q, of heat can actually produce is $Q \times \frac{T - t}{T}$, or the efficiency of all such engines cannot be greater than $\frac{T - t}{T}$.

Now, in the steam-engine the temperature of vapour in the cylinder is somewhat less than in the boiler, and some portion of the heat is lost by conduction and by frictions; also the pressure of steam increases so rapidly with temperature that the temperature attainable is limited from the danger of explosion of the boiler. By employing superheated steam separated from the water in the boiler, a higher temperature may be attained and the efficiency of the engine greatly increased. For high-pressure engines the usual temperature is not higher than 150° C., so that

the range of temperature is only about 100° C., and the efficiency certainly less than $\frac{100}{423}$, or ·236, *i.e.*, less than 24 per cent. This is the greatest efficiency which would be possible in a perfect engine, working between the temperatures of 150° C. and 50° C., supposing the temperature of the steam to be lowered by its expansion in the cylinder to the temperature of 50° C. This would mean that the steam must be allowed to expand to twenty-six times its original volume, which would be impracticable, and, if it were possible, would introduce other serious causes of loss of energy.

The temperatures between which steam-engines have been actually worked are such as to give a theoretical efficiency of from 30 to 33 per cent., supposing the engines to be perfect engines, *i.e.*, engines in which all the heat applied is converted into useful work. The actual efficiency is very much less than this: the best steam-engines have only a practical efficiency of from 10 to 13 per cent.

The question whether the hot-air-engine will offer greater advantages than the steam-engine must depend upon whether it can be worked at a very high temperature or not, for only in that case can its efficiency be greater.

With the Stirling hot-air-engine the temperature is such that a pressure of about 37 lbs. per square inch is obtained, and the efficiency has been found to be about 30 per cent. But the difficulties of heating the air, without raising the temperature of the cylinder so high that it was burnt out, have prevented this engine from being much used.

Gas-engines.—In connection with the production of electrical energy, considerable attention has for some time past been given to another form of heat engine, in which an explosive mixture of ordinary coal gas with air is compressed and then exploded inside a cylinder. By this means very high temperatures may be reached, and hence a considerable amount of efficiency may be expected to be attained.

In the working of the Otto gas-engine, the forward motion of the piston draws in air and an explosive mixture of air and gas;

then the return of the piston compresses this mixture into less than half the length of the cylinder, the charge is then ignited and the explosion takes place, raising the temperature and pressure to their highest value; the piston is driven forward, and the pressure falls, then on the return stroke the exploded mixture is driven out of the cylinder. The work done in the explosion and expansion of the gases occupies less than a quarter of the period of two revolutions.

In the Clerk gas-engine a second cylinder is added to supply the explosion cylinder with the explosive mixture, so as to get an explosion at every revolution. During the explosion and expansion in the explosion cylinder a piston has been drawing in first air and gas and then air into the second or displacer cylinder, and at the beginning of the return stroke they are driven into the explosion cylinder, the air first to clear out the used gases, and then the explosive compound, which is compressed at the end of the stroke to a pressure of nearly three atmospheres. The explosion then takes place, raising the temperature and the pressure to their highest value, and the piston is driven forward.

In these gas-engines, although there is great loss of heat through the cylinder, which must be surrounded with a water jacket with a constant supply of cold water, yet the form of the curve of pressure keeps nearly up to the isentropic line, as if no heat were allowed to escape. This is accounted for by supposing that the temperature of dissociation of the gases is reached, or that at the temperatures reached a portion of the gas is dissociated, which enters into combination again when the pressure is gradually diminished by the forward motion of the piston. In this way the gas gradually burnt supplies the heat lost through the sides of the cylinder. Now the temperature may possibly reach to 1,800° or 2,000° C., and indeed in gas-engines has been found to reach 1,530° C.; hence considerable efficiency may be attained, notwithstanding the fact that the exploded mixture would have its pressure somewhat diminished by an amount depending upon the contraction in volume of the gases forming the explosive mixture when they are exploded under constant pressure. With hydrogen, or with a mixture of hydrogen and carbonic oxide only, burnt in

oxygen, this contraction would amount to one-third of the whole, and so the pressure attained would only be two-thirds of the pressure, supposing there were no contraction. If other gases are mixed with these, as in ordinary gas, then the pressure will not be so much reduced from this cause, but there will always be some such reduction from the pressure which would otherwise be attained when the gases are exploded. Were there no dissociation, a very much higher temperature would be attained than is actually observed in gas-engines.

Making the supposition that the highest temperature reached is about 1,530° C., and that explosion takes place at once through the whole of the space occupied by the gas, the pressure at the first instant after explosion would be five or six times the pressure before explosion. If the gases be compressed to a pressure of nearly three atmospheres before explosion, the pressure of the exploded mixture is at first about fifteen atmospheres at a temperature of 1,500° C. Supposing that the expansion of the exploded gases is allowed to continue until the pressure is equal to the pressure of the atmosphere, the volume of the expanded gases would be more than six times their volume before expansion; and the temperature of the mixture, supposing no heat to have been lost through the walls of the cylinder, would be reduced to about 370° C.

As it would be inconvenient to go beyond the volume at which the mixture before explosion was at atmospheric pressure, —*i.e.*, where the ratio of volumes is 3 to 1,—under these circumstances the temperature at which the products would be discharged from the cylinder would be about 1,060° C. Taking into account the loss of heat by conduction and frictions, the temperature would be considerably lower. This would give an efficiency of about 25 per cent. With very high temperatures, such as these in gas-engines, the loss through the sides of the cylinder must be very great, for the cylinder must be kept cool in order that the piston may work in it, so that it is impossible to make use of the principle of the regenerator in this form of gas-engine to heat up the entering gases.

Let me illustrate my remarks on this subject by some results

obtained from the experimental tests made on gas-engines at the Crystal Palace.

Assuming the values given by Mr. Clerk, in his paper before the Institution of Civil Engineers, for the heat evolved on the combustion of ordinary gas, we may arrive at the practical efficiency of the Otto gas-engine. One pound of gas at 17° C. measures 35·5 cubic feet, and evolves on combustion 12,500 units of heat.

Now, in the Crystal Palace experiments, with the 12 H.P. Otto gas-engine, the quantity of gas used per hour was 533·4 cubic feet, or 15 lbs. Hence the heat absorbed per minute is $\frac{12500 \times 15}{60}$, or 3,125 units. The work done per minute upon the piston (which is 12 inches in diameter, and has a 16-inch stroke), at the rate of 158·7 revolutions per minute, with a mean pressure of 62·2 lbs. on the square inch, is $\frac{158{\cdot}7}{2} \times 62{\cdot}2 \times \frac{4}{3} \times \frac{22}{7} \times 36$, or 744,800 units of work. This is equivalent to 536 units of heat. Hence the theoretical efficiency is $\frac{536}{3125}$, *i.e.*, ·1715, or 17 per cent.

The available indicated H.P. is 22·56, and the H.P. as shown by the Froude dynamometer is 18·31.

Hence the practical efficiency $= \frac{1831}{2256} \times {\cdot}1715 = {\cdot}1392$, or 14 per cent.

The gas used per brake H.P. is 29·1 cubic feet.

With the 16 H.P. gas-engine the quantity of gas used per hour was 841·6 cubic feet, or 23·71 lbs.

Hence the heat used up per minute is $\frac{12500 \times 23{\cdot}71}{60}$, or 4,940 units; the diameter of piston being 13 inches, length of stroke $1\frac{3}{4}$ feet, mean pressure 63·08 lbs. per square inch, revolutions 151·37 per minute.

The work done per minute upon the piston is $\frac{151{\cdot}37}{2} \times \frac{7}{4} \times 63{\cdot}08 \times \frac{22}{7} \times \left(\frac{13}{2}\right)^2$, or 1,109,000 units of work, which is equivalent to 798 units of heat.

Hence the theoretical efficiency is ·1615, or 16 per cent.

The available indicated H.P. is 33·6.

The H.P. on the brake dynamometer is 27·75.

Hence the practical efficiency is $\frac{2775}{3360} \times \cdot 1615$, *i.e.*, ·1334, or $13\frac{1}{3}$ per cent.

The gas used per brake H.P. is 30·3 cubic feet.

It will be seen that 82·6 of the indicated H.P. appears on the dynamometer.

With the 2 H.P. Otto gas-engine, the quantity of gas used per hour was 95·8 cubic feet, or 2·7 lbs.

Hence the heat generated per minute is $\frac{12500 \times 2\cdot 7}{60}$, or 562·5 units.

The diameter of the piston is 5·75 inches, length of stroke 1 foot, mean pressure 54·31 lbs. per square inch, revolutions 160·3 per minute.

The work done per minute upon the piston is

$$\frac{160\cdot 3 \times 1 \times 54\cdot 31 \times 22 \times (5\cdot 75)^2}{2 \times 7 \times 4} = 113{,}100 \text{ units of work}$$

nearly, which is equivalent to 81·37 heat units, or to 3·42 H.P.

Hence the theoretical efficiency is ·1446, or $14\frac{1}{2}$ per cent.

The H.P. on the brake or dynamometer is 2·87.

Hence the practical efficiency is $\frac{287}{342} \times \cdot 1446$, *i.e.*, ·1214, or 12 per cent.

The gas used per brake H.P. is 33·4 cubic feet.

It will be seen that 84 per cent. of the indicated H.P. appears on the dynamometer.

It would appear, then, from these results, that even in the early days of gas-engines each of these gas-engines is superior in practical efficiency to the very best steam-engines.

It must be taken into consideration that these efficiencies depend upon the quality of the gas, so that, if the gas used at the Crystal Palace was not of sufficiently good quality to yield 12,500 heat units per lb. on consumption, these estimates of efficiency are too low.

Experiments were made with both the Otto and the Clerk engines, with different quantities of gas and also with different loads on the dynamometer. In working with the Otto 16-H.P. engine with a full load, it was found that after some time the

cylinder became so hot that no flame was required to inflame the gas. A series of diagrams was taken with no gas jet to explode the gas, the explosion being caused by the heat of the cylinder when the unexploded gases were compressed by the piston. Fig. 1 (a) shows the character of the indicator diagram under these circumstances.

The Clerk engine was worked not only with a full load, for which Fig. 2 shows the form of the indicator diagram, but with a medium load and also with a light load, and Fig. 2 (a) shows the form of the indicator diagram when there is an explosion at every revolution with a light load.

The indicator diagram for the displacer cylinder with a full load is given in Fig. 3 (a).

The Froude's absorption dynamometer employed in these experiments was kindly lent, and its working was superintended during the experiments by Mr. R. H. Froude.

The available horse-power in the Otto gas-engine and in the Clerk engine have been calculated from the indicator diagrams, of which specimens are given in Figs. 1 and 2.

In these figures the circle shows the motion of the crank pin, and the distances between the ordinates are approximately the distances described in equal times by the piston. The periods of explosion, expansion, exhaust, indraught, and compression can readily be obtained from the figures.

Fig. 3 is a diagram (drawn by Mr. J. T. Sprague) to show the relative positions of the pistons, and the relative pressures at the same instant in the explosion cylinder, and in the displacer cylinder of Clerk's gas-engine at different parts of the stroke.

The following are the general conclusions with regard to the working of gas-engines, which have been drawn from the consideration of a great many indicator diagrams, taken under different conditions :—

The pressure curve almost coincides with the isentropic line of gases.

With perfect ignition the rate of increase of pressure is very nearly uniform up to the maximum, which is reached in about the one-thirtieth of a second.

That the maximum pressures obtained in successive strokes are not the same, nor the times required for the maximum.

That the latter halves of the pressure curves are identical, provided the maximum pressure is reached at any time during the first half of the stroke. This is the case, whether the explosion takes place promptly or slowly.

That the greatest amount of work is done when the maximum pressure is reached at the beginning of the stroke.

That, with a very hot cylinder, the heat developed on compressing the gases may be sufficient to cause the explosion without a flame to fire the gases, and that even the greatest pressure may be reached before the return stroke is finished.

That the greatest pressure reached in large and small engines is practically the same (considering the amount of compression), but that the time of reaching the greatest pressure is somewhat less in small engines than in large ones.

With higher compression of the gases before ignition, the maximum pressures are increased, and the efficiency of the engine is greatly increased.

A considerable amount of loss of energy may arise from not carrying the expansion far enough before discharging the exploded gases. In the Clerk engine, especially, these gases were discharged at too high a pressure and temperature. The higher compression, which is practicable on account of the greater number of explosions in a given time in this engine, should greatly add to its efficiency. The engine works far more regularly in consequence of having an explosion at every revolution.

The results of the tests and the form of the indicator diagrams agree best with the theory that in gas-engines dissociation takes place in the cylinder, and the pressure is kept up by continued combustion of the gases as the temperature falls.

It was found that from the beginning of the rise of pressure to the time of reaching the maximum pressure, except in certain special experiments when different quantities of gas were used, the interval was scarcely ever more than one-thirtieth of a second, and was often not more than one-fiftieth of a second. In exceptional cases the ignitions took place very late in the stroke, and the maximum pressures obtained were very much reduced.

In all the experiments with Crossley's Otto engine (whether large or small), the maximum pressures were higher as the time of reaching the maximum was diminished.

There are manifestly two very great defects in gas-engines—1. One-half of the heat resulting from the explosion of the gases passes away through the sides of the cylinder and is thus entirely lost in heating the cylinder. 2. The exploded gas is discharged from the engine at a temperature of about 1,000° C. or 900° C., and so carries away with it a very large portion of the remainder of the heat due to the explosion of the gases.

As pointed out by Sir William Siemens in the discussion on "Gas-Engines" before the Institution of Civil Engineers, this heat should be saved by communicating it to the incoming gases, so that their temperature before combustion should be 1,000° C. instead of 60° C. This is impracticable in the present form of gas-engine, in which a piston works within the cylinder. In 1860, Sir William Siemens had constructed a gas-engine in which compression was employed, and in which the heat from the used-up gases was communicated to the incoming gases, thus carrying out the principle, which with him was of universal application, of using up, as far as possible, energy which would otherwise escape as wasted energy. In this engine the combustion of the gases took place as they entered a cylinder under compression, without working a piston: the cylinder could be kept hot, so that the heat of the gases would not be lost.

Efficiency of Dynamo Machines.—Having considered the efficiency of steam-engines and other heat engines, by means of which heat may be converted into energy of motion or mechanical work, I wish now to consider the conversion or transformation of mechanical work into electrical energy by means of dynamo machines, and to illustrate my remarks by the results of the experimental tests of dynamo machines made at the close of the Crystal Palace Exhibition.

In this, as in other branches of the subject, accurate measurement and practical applications react on theoretical investigations in supplying the instruments and the means of progress. The dynamo machine has supplied the means of testing and working

out its own theory; and even the Edison machine may be improved, when the theoretical and practical knowledge of a Hopkinson are brought to bear upon it.

In the electric circuit of a dynamo machine with an internal resistance a (the armature) and f (the field), and external resistance r, the whole arranged in simple circuit, we have, by Ohm's law, $E = C\,(a + f + r)$, and the difference of potential, p, at the terminals of the machine, $p = C\,r$. The electrical energy in watts $= E\,C$ or $= C^2(a+f+r)$. This is converted into heat.

For shunt dynamo machines the total current, C, is the sum of the currents in the field magnets, C_1, and in the external circuit, C_0. The E.M.F. of the machine $E = C\left(a + \frac{f\,r}{f + r}\right)$, and the electrical energy in watts, E C, since $C = C_0 + C_1$. The electrical energy spent in heating the armature $= C^2\,a$. The electrical energy or heat in the magnets $= C_1^2 f$. The electrical energy in the external circuit $= C_0^2\,r$ where $p = C_1 f = C_0\,r$.

The work applied to the dynamo machine must be determined by a transmission dynamometer. The *electrical efficiency* of the dynamo is the ratio of the total electrical energy to the total energy absorbed, deducting the energy spent on the friction of the driving shaft.

Of the total electrical energy produced, one portion is spent in heating the wire of the armature and field magnets, and is therefore wasted; the remainder is spent on the external circuit, and is the only portion which can be converted into useful work. The ratio of this external electrical energy to the total energy absorbed may be termed the *useful electrical efficiency* of the machine. In order to determine the efficiency of a machine, (1) the internal resistances of the armature and field should be determined, (2) the difference of potential at the terminals, (3) either the current or the external resistance. Then the external resistance or the current respectively may be deduced from Ohm's law, and the electrical energy in *watts* may be determined.

In the Crystal Palace experiments (see *Supplementary Note* and Fig. 5) the *difference of potential* at the terminals was found by charging a condenser from them and discharging it through a

high-resistance galvanometer, and comparing the deflection with the deflection obtained by charging the same condenser from a Clark's standard cell. *The current* was obtained (1) by charging the condenser from the extremities of a given resistance consisting of several branches forming part of the circuit; (2) by means of a tangent galvanometer, N, placed in one of these branches, whose resistance was known. The work absorbed per second was determined, and is expressed in watts, so that it may be readily compared with the electrical energy.

In the Table of Efficiencies (p. 46A), when three values are given for the current in any dynamo machine, the first is the value given by the tangent galvanometer, and the third by the condenser, the middle value being the mean of the two values so determined.

Let me work out one case completely, so as to show the methods adopted to determine the efficiency of each machine.

I will select one of the three Bürgin machines (the C_3 machine) submitted by Mr. Crompton. In the C_3 machine the resistance of the armature (cold), a, = ·528 ohm; the field magnet, f, = ·435 ohm; the difference of potential, p, = 92·8 volts; and the current, 31·18 ampères.

To determine the difference of potential at the terminals—

Let d be the deflection of the galvanometer on discharging a condenser after charging it from the standard cell, let B be the mean deflection on discharging the condenser after charging it from the machine terminals, t the ratio between the values of the shunts employed; then $p = \frac{t\,B}{d} \times 1{\cdot}457$ volts. In this case $t = 10{\cdot}47$, $B = 219{\cdot}1$, $d = 36$; hence $p = 92{\cdot}843$ volts.

To determine the current C in the external circuit—

(1) *By the Condenser.*—Let b be the mean deflection on discharging the condenser after charging it from the terminals of the tangent galvanometer circuit, and m_1 the resistance (hot) of wires on the frame; then $C = \frac{b \times 1{\cdot}457}{d\,m_1}$. In this case $b = 148{\cdot}6$ and $m_1 = {\cdot}158$; hence $C = 38{\cdot}064$ ampères.

(2) *By the Tangent Galvanometer.*—Let θ be the deflection of the tangent galvanometer, k its constant, g its resistance, m the resistance of wires used in the experiment; then

$C = \dfrac{\tan.\theta \times k \times g}{m_0}$. In this case $\theta = 47{\cdot}67°$, $k = 3{\cdot}469$, $g = 1{\cdot}559$, $m = {\cdot}155$; hence C = 38·30 ampères.

The mean value, C_0, from these two results is 38·182, the external resistance, $r, = \dfrac{p}{C_0} = \dfrac{92{\cdot}843}{38{\cdot}182} = 2{\cdot}4316$ ohms, the internal resistance $(a + f)$ of armature and field when hot from the current is 1·02 ohms; hence the whole resistance is 3·4516 ohms, and the whole electro-motive force of the machine $= C_0(a + f + r) = 131{\cdot}79$ volts.

Now with regard to the efficiency of this machine, the number of watts $= C^2(a + f + r) = 5{,}031{\cdot}5$, and the electrical horse-power $= \dfrac{C^2(a + f + r)}{746} = 6{\cdot}7461$.

Now the actual horse-power applied as measured by the dynamometer, deducting the horse-power required to overcome the friction at the driving speed, is 7·7018; hence the electrical efficiency of the machine, *i.e.*, the fraction of the power applied which appears as power in the shape of electric current is $\dfrac{6{\cdot}7461}{7{\cdot}7018}$ $= {\cdot}8759$, or nearly 88 per cent. Of this, the fraction which appears in the external circuit is the ratio of the external resistance to the total resistance, $= \dfrac{2{\cdot}4316}{3{\cdot}4516} = {\cdot}70448$. Hence the external or the useful electrical efficiency is 61·7 per cent.

On comparing the two methods of measuring the current, we have the following results:—

By Condenser Method.

C = 38·064.

$r = \dfrac{92{\cdot}843}{38{\cdot}064} = 2{\cdot}4391$.

Total resistance = 3·4591.

E.M.F. = C × 3·4591 = 131·67.

Units of work, or watts, 5012.

Electrical H.P. = 6·7191.

H.P. applied, deducting for friction, = 7·7018.

Electrical efficiency = ·87241.

The fraction of this appearing in the external circuit = ·70513.

External electrical efficiency = ·6152, or 61·52 per cent.

By Tangent Galvanometer.

C = 38·30.

$r = \dfrac{92{\cdot}843}{38{\cdot}30} = 2{\cdot}4241$.

Total resistance = 3·4441.

E.M.F. = 131·91.

Watts = 5053.

Electrical H.P. = 6·7731.

H.P. applied, deducting for friction, = 7·7018.

Electrical efficiency = ·87943.

Portion in external circuit = ·70383.

External electrical efficiency = ·6190, or 61·90 per cent.

Thus the difference in the two methods is only about two-thirds per cent. on the efficiencies.

For machines with a separate exciter, such as the Maxim machine and the H Gramme machine, the same methods were adopted for finding the current and the potential at the terminals, but in the case of the exciter the current was obtained from the potential at two points, and the resistance measured between them by the Wheatstone bridge immediately after the experiment whilst the portion of the circuit was still warm.

In shunt dynamo machines the potential was determined at the terminals, and the current in the field-magnets determined from their resistance measured (warm) by the Wheatstone's bridge immediately after the experiment. The current in the external circuit being determined by the same methods as before, and the external resistance when running deduced from this current. In this case the total resistance is the resistance of the armature together with the combined resistance of field and external circuit. The total current, C, is the sum of these two currents. Hence the E.M.F. of the machine $= C\left(a + \frac{f\,r}{f + r}\right)$ where $C = C_0 + \frac{p}{f} = C_0 + \frac{t\,B}{f\,d} \times 1{\cdot}457$.

It will be seen from the table of efficiencies of these dynamo machines (see p. 46A) that there are several which are capable of converting into electrical energy more than 80 per cent. of the energy absorbed, and that some convert more than 90 per cent., so that as converters of energy they are very efficient machines. But in some of these the internal resistance is so high as compared with the external that there are only five which give more than 80 per cent. of their electrical energy in the external circuit —*i.e.*, the useful electrical energy is only 80 per cent. of the total electrical energy.

The Edison machine, with thick copper rods or bars in the armature, converts into electrical energy 91 per cent. of the absorbed power, and of this amount 91 per cent. appears in the external circuit, so that $\frac{91}{100} \times \frac{91}{100}$, or ·83 of the absorbed power appears as useful electrical energy in the external circuit.

Of the others there are only three, viz., the B Gramme, the Maxim, and the Weston, which convert over 70 per cent. of the power applied to them into useful electrical energy.

After discussing the Bürgin C_2 machine so fully, it is only fair to Mr. Crompton that I should state that since these tests were made he has entirely altered the internal resistance of his machines and their general arrangement, and has greatly improved their total efficiency as well as their useful electrical efficiency.

Through the kindness of Dr. Hopkinson I am able to give you the latest information with regard to the Edison-Hopkinson dynamo machines. One of these machines was tested by Mr. Sprague after our tests were made at the Crystal Palace, and he found that 94 per cent. of the absorbed energy appeared as electrical energy. A large Edison-Hopkinson machine on board the "Oregon" supplies 520 Edison A lamps with a current of ·72 ampères each, and the E.M.F. is 107 volts, so that the amount of useful energy is 40,000 watts. The resistance of the field-magnets is 17 ohms, and of the armature less than ·01 ohm when warm; hence the current in the magnets is about 6 ampères, and the total current in the armature 380 ampères. The energy spent in heating the field magnets is $\frac{107^2}{17} = 670$ watts; in the armature is $380^2 \times \cdot 01 = 1{,}444$ watts; thus only 6 per cent. of the electrical energy is spent in heating the machine, and 94 per cent. of the electrical energy appears as useful electrical energy in the external circuit.

Thus $\frac{94}{100} \times \frac{94}{100}$, or ·88 of the power absorbed, is converted into useful electrical energy. The 83 per cent. efficiency of the Edison machine has been raised to 88 per cent. by the improvements introduced into it by Dr. Hopkinson.

In considering these efficiencies of dynamo machines, as well as all efficiencies which have been determined up to the present time, it is important to remember that they depend upon the value of our unit of resistance in two ways—(1) directly, because we are measuring resistances, and (2) indirectly, because in measuring electro-motive forces we take as our standard a unit of

electro-motive force which depends upon the value of our unit of resistance.

Thus Clark's standard cell is said to have an electro-motive force of 1·457 volts, but this number is derived from the B.A. unit of resistance, which several experimental tests by Lord Rayleigh and by others have shown to be ·9865, or more than 1 per cent. below the theoretical unit of resistance. The E.M.F. of Clark's cell and of standards of E.M.F. must be altered to the same extent to suit the Rayleigh ohm. Hence all efficiencies of dynamos which have been hitherto determined must be more than 1 per cent. too high, and should be reduced in the ratio of ·9865 to 1.

Having considered the efficiency of the dynamo machines for converting mechanical energy into electrical energy, which may be regarded as another form of energy of motion, we have now to consider the reconversion of this electrical energy back again into heat and into energy of radiations, as we compare the candle-power with the energy expended under different conditions in incandescent lamps.

Method of Testing the Incandescent Lamps.—A current from a Grove's battery was sent through the lamp to be tested and the current-meter in simple circuit, the lamp being in its place on the Bunsen photometer. Wires were carried from the two electrodes of the lamp to the key employed for discharging the condenser through the reflecting galvanometer, and wires from the same electrodes were also connected to the voltmeter. The positions for the equality of illumination in the photometer were found for red light and for green light by looking at the photometer through red and green glasses respectively. The illuminating power of the standard lamp with which the incandescent lamps were compared was determined to be 17·5 candles. At first a set of observations was made simultaneously by the different observers with, say, 20 cells of Grove, then 5 cells more were added and the observations repeated, then 5 cells more, and so on up to 40 or 50 cells or higher. In some cases experiments were carried to the breaking point, or giving way of the lamp, when it was found that the lamps of each maker had their own distinctive peculiarity.

By making the tests in this way we were able to determine the characteristic, and to plot the characteristic curve for each lamp—a curve in which the energy absorbed is measured horizontally and the candle-power measured vertically.

The values of $\frac{E}{C}$, or the resistance of the lamps, show clearly that there is a point which is reached in the case of the Edison lamps when their resistance diminishes no further, but begins to increase as the current increases. If we draw the curve with the current for the abscissa, and the E.M.F. for the ordinate (Fig. 6), we see where the resistance begins to increase by a change in the direction of curvature of the curve. Probably at this point the carbon filament begins to give way at the junction with the copper, for it is generally found that this is the point which first yields in the Edison lamps. In the case of the Swan lamps this critical point was not reached, and the curvature continues to be in the same direction to the extreme point tested.

The characteristic curves for the principal incandescent lamps are given in Fig. 7, and the efficiencies of the different groups of lamps may be found by measurement from these characteristic curves: the values of electro-motive force being the mean of the values given by the voltmeter and by the discharge from the condenser; E C, the energy expended, being the product of the values of the current and electro-motive force so determined.

The characteristic curve for the Edison B lamps shows that at 10 candle-power the Edison lamp yields 150 candles per horse-power. As the illumination is increased the yield increases. At 16 candle-power there are 5·7 kilogrammètres and 211 candles per horse-power. At 32 candle-power there are 7 kilogrammètres and 342 candles per horse-power. At 40 candle-power 7·5 kilogrammètres are absorbed and there is a yield of 400 candles per horse-power. Comparing this with the Swan lamps, we see that an absorption of 7·5 kilogrammètres gives 60 candle-power in each lamp and a yield of 600 candles per horse-power. The characteristic curve for the Swan lamps shows that at 10 candle-power there is a yield of 200 candles per horse-power. At 16 candle-power there are 4·4 kilogrammètres absorbed and a

yield of 272 candles per horse-power. At 32 candle-power there are 5·8 kilogrammètres absorbed and a yield of 415 candles per horse-power. I have employed these numbers for the sake of comparison with the results arrived at in Paris by Professor G. F. Barker and Mr. Crookes, and given in Vol. XI., p. 229, of our Journal. As might be expected, the economy of all incandescent lamps is greater at high than at low incandescence. This might be expected, since there must be a certain quantity of energy used up in the radiation of dark heat from the carbon filament, when the temperature is not high enough to make it glow, and this dark heat is not increased so greatly in amount when the filament has reached the temperature at which it gives a brilliant light. The comparison of the characteristic curves of Swan high-resistance and low-resistance lamps with Edison B lamps does not seem to support the statement that "the economy of light production is greater in high-resistance than in low-resistance lamps;" for the economy of Swan No. 13, whose resistance cold is 194 ohms, and Swan No. 16, whose resistance cold is 20 ohms, are very nearly the same, the curves crossing one another; and both of these are more economical than Edison B lamp, whose resistance cold is 136 ohms.

These experiments clearly show that the economy of light-production is not necessarily greater in high-resistance than in low-resistance lamps, but is rather a function of the material of the filament and the way in which that filament is made. Swan's 200-ohm and his 20-ohm lamps are shown to be nearly equal in efficiency, the resistance of the first when giving an illumination equal to that of 37 candles being 102 ohms, and of the second less than 10 ohms. The other low-resistance lamp, which is nearly of the same efficiency and more efficient than any of the Edison lamps or of the lamps of other inventors, has a resistance of about 13 ohms when giving the same illumination of 37 candles.

These results show that the question of constructing incandescent lamps of high or low resistance must be decided entirely by the economy of distribution of electricity. In the case of the high-resistance lamp, the current for 37 candles is only about three-fourths of an ampère, whereas the current for the same illumina-

tion from the low-resistance lamps is at least three times as great, or $2\frac{1}{4}$ ampères.

Transmission of Power by Dynamo Machines.—We have still to consider the reconversion of electrical energy back into mechanical work by means of a motor dynamo machine. If two dynamo machines be placed on the same electric circuit, and one of them be set in motion to generate a current of electricity, part of the energy of the current will be spent in heating the conductor throughout the circuit, and another portion will be spent in setting the other dynamo in motion in such a direction as to produce an opposing electro-motive force in the circuit.

The previous equations for dynamos are modified, and the extension of Ohm's law gives $E = e + C R$, where e is the opposing electro-motive force of the motor dynamo and R the total resistance of the circuit. We have also the equation $E C = e C + C^2 R$ connecting the different portions of the electrical energy. Here $e C$ is the number of units of electrical energy which may be converted into mechanical work by means of the axle of the motor dynamo. The ratio, $\frac{e}{E}$, may be termed the electrical efficiency of the combination. The equation $e = E - C R$ shows that for the same electro-motive force of the generator, *i.e.*, for the same rate of revolution, the contrary electro-motive force of the motor, and consequently the speed of the motor, diminishes as the resistance of the circuit increases. The effective work, e, C is greatest when $e C = C^2 R$, *i.e.*, when the electrical efficiency, $\frac{e}{E}$, is $\frac{1}{2}$, *i.e.*, when one dynamo revolves at half the speed of the other, supposing the dynamos to be alike.

The electrical efficiency will then be the ratio of the rates of revolution. So much progress has been made in the reconversion of electrical energy into mechanical energy, or energy of motion, during the past year, that I must include some notice of it in my address.

The use of dynamo machines for the transmission of power to considerable distances has now been accomplished, the loss in converting electrical into mechanical energy being no greater than in the reverse action. Electrical and mechanical energy

being two modes of the energy of motion which are mutually convertible, the only loss is that due to friction and the heating of the wires. We have seen that the external or commercial efficiency of the best dynamo machines is 88 or 89 per cent.—say, 88 per cent. Taking the same efficiency for reconversion, the twofold conversion may be effected with an efficiency of 77·5 per cent., *i.e.*, with a loss of 22·5 per cent. But the conductors will absorb some energy as heat through their electrical resistance, the amount depending on the length of the conductors and on the current which they have to carry, *i.e.*, upon the work which the motor is required to do: allowing 5 per cent. for this will still leave a mechanical efficiency of 72 per cent.

Visitors to the Electrical Exhibition in Paris cannot fail to have been struck with the great variety of applications of electrical energy which had already been made by M. Marcel Deprez, and they will not have been astonished at the later results at Munich, again at Paris at the Chemin de Fer du Nord, and the much greater success attained within the last few months at Grenoble.

The objections which were urged with regard to the two machines in the experiments at the Chemin de Fer du Nord in Paris, that they were placed close together, can have no place in the Grenoble experiments, where the dynamo machines were placed 14 kilomètres, or nearly 9 miles, apart. In these experiments wires of the new material, *siliceous bronze*, of 2 millimètres diameter, were employed as the conductors, and greater attention was paid to the insulation of the two machines. The power was applied to the generating machine from a turbine, and was measured from time to time by means of a Prony friction-brake, the velocity of the turbine being the same as in the running of the dynamo machine.

As one result of the experiments carried out at Grenoble, it has been shown, experimentally, that the efficiency increases with the velocity of rotation of the generating machine (as the formula $\rho = \dfrac{1}{1 + \dfrac{R}{n\,(a + b\,m + c\,m^2)}}$ would give). These experiments have also shown an electrical efficiency of from

60 to 70 per cent., corresponding to a commercial or mechanical efficiency of conversion of from 50 to 62·3 per cent.

In these experiments the velocities of revolution were so high that the magnetic fields of the machines were near their points of saturation, and so might be regarded as no longer varying with the current.

The opening of the Portrush Electric Railway in September last, and its practical working, show that in our own country Sir William Siemens has not been behind in the practical applications of electrical energy. A separate conductor is placed at the side of the railway to carry the current, the return circuit being completed through the rails themselves. The power is obtained by means of turbines from a waterfall with a head of 24 feet. The electrical conductor is maintained at an electro-motive force of 225 volts. The resistance of the conductor per mile is ·23 ohms; and, with four cars running, requiring 4 horse-power each, the loss due to resistance does not exceed 4 per cent. of the power developed on the cars. In his paper on this railway, Mr. A. Siemens has shown the use which may be made of characteristic curves of dynamos, when the object is to find what arrangement of dynamo machines is best adapted to perform any given work.

We see, then, that by means of the dynamo machine mechanical work may be converted into electrical energy, and that in the form of the electric current as much as 88 per cent. of the total energy expended can be usefully employed in the external electric circuit in producing heat (as in the electric furnace) or light (as in arc and incandescent lamps). Or this energy may be conveniently transmitted to a distance from its source, and may then be converted back into mechanical work, giving out again more than 60 per cent., or even 70 per cent., of the mechanical work originally spent in its production. We may see the dynamo machine

> Piercing with the little diamond
>   Deep into the mountain side;
> Then, retiring, give the spark,
>   And with a shock the rocks divide:
> Then he lifts the heavy masses,
>   Melts and moulds the iron way,
> Sheds his light, and with his burden
>   Rushes to the open day.

SUPPLEMENTARY NOTE.—THE TESTING OF DYNAMO MACHINES AND INCANDESCENT LAMPS.

The experiments at the Crystal Palace on gas-engines and on dynamo machines were carried out by the same Committee, consisting of Mr. Horace Darwin, Mr. F. J. Sprague, Mr. Spagnoletti, Mr. Crampton, and myself—Mr. F. J. Sprague devoting himself most zealously to the work.

The engine supplying the power to drive the dynamo machines was kindly lent by Messrs. Davey, Paxman, & Co. It was a semi-fixed single-cylinder engine, which ran very regularly, and was capable of giving from 40 to 43 H.P. in the cylinder, and did not use in the various experiments more than from $1\frac{1}{2}$ to 3 H.P. for friction. The transmission dynamometer and the electrical apparatus for testing were kindly lent by Messrs. Latimer Clark, Muirhead, & Co.

The Dynamometer (Fig. 4).—The two parts of the belt from the fly-wheel of the engine passed between two nine-inch pulleys, A A, which are mounted in a frame attached to a fixed standard, to the pulley Q on the shaft. The frame supporting the pulleys was supported at one end of a long lever, U T, and counterpoised. The lower belt was the driving belt, and the pull upon the end of the lever was equal to the difference of the tensions of the lower and upper belts resolved at right angles to the line joining the centres of the fly-wheel and the shaft Q. The centre of the fly-wheel was nearly on a level with the floor, so that the line joining the centres of the fly-wheel and shaft was inclined 2° to the horizontal. The lineal speed of the belt varied from 44 to 51 feet per second, and the difference of tension on the two parts of the belt was as much as 250 to 300 lbs. for the heavier loads. Near the pulleys A A, and between them and the shaft, a wooden frame was erected, consisting of two uprights on opposite sides of the belt, with a cross-piece. This was the support for a lever, U T, made of iron of an inverted T section. Through this were secured square bars of iron, U and u_1, for knife-edges. To the one at the end of the lever u_1 was hung a shackle connected with a link, K, and side-adjusting screws, supporting the pulley frame. The

distance between these knife-edges was 2′ 6″. The other end of the lever was prolonged about 10 feet, and the end allowed 3 to 4 inches play each side of the horizontal by wooden bars. When horizontal, the pulley frame was very nearly in its true middle position, and a deviation of one inch either way sufficient to throw the end of the lever to the extreme limit allowed. With a load on, the lever being horizontal, a slight pressure of the finger would destroy the equilibrium. The weight of the frame and pulleys, and the drag of the lower belt when at rest, was counterbalanced by the weight J.

To determine the constant for this dynamometer, and to consider its accuracy,

Let R = radius of fly-wheel = C E = $42\frac{1}{2}''$;

r = radius of shaft pulley = Q b = $14\frac{1}{2}''$;

C = distance between faces of dynamometer pulleys mm.′ = $1\frac{1}{4}''$;

k = thickness of belt;

D = distance of centre of fly-wheel to middle of dynamometer = C E = 20′ 9″;

d = distance from centre of shaft pulley to dynamometer = Q E = 7′ 6″;

t and t_1 = tension of lower and upper belts;

p and p_1 = corresponding pulls at right angles to line of centres;

P = resultant *vertical* pull;

l and l_1 = short and long arms of lever;

$2\,a$ = right enclosed angle;

$2\,\beta$ = left enclosed angle;

s = speed per minute of rim of shaft pulley;

n = number of revolutions of shaft pulley per minute;

W = balancing weight at distance of 1 foot;

θ = 2° 36′ = the angle of inclination of line of centres to the horizon.

We have

$$a = \sin.^{-1}\frac{\mathrm{R} - \frac{1}{2}(c - 2\,k)}{\mathrm{D}} = \sin.^{-1}\frac{\mathrm{R} - \frac{1}{2}c + k}{\mathrm{D}} = 9^\circ\ 45'.$$

$$\beta = \sin.^{-1}\frac{r - \frac{1}{2}c + k}{d} = 9^\circ\ 4'.$$

$$p = t\,(\text{sin. } a + \text{sin. } \beta) \quad \cdot \quad p_1 = t_1\,(\text{sin. } a + \text{sin. } \beta).$$

$$\text{P} = \frac{1}{l}\,\text{W} = (p - p_1)\,\text{cos. } \theta = (t - t_1)\,\text{cos. } \theta\,(\text{sin. } a + \text{sin. } \beta).$$

Whence

$$(t - t_1) = \frac{1}{l} \cdot \text{W}\,\frac{\text{sec. } \theta}{\text{sin. } a + \text{sin. } \beta}\,.$$

The work transmitted to the shaft pulley per minute is

$$(t - t_1)\,s = (t - t_1) \cdot \frac{2\,\pi\,r\,n}{12}.$$

Now sec. $\theta = 1$ nearly, and

$$\text{sin. } a + \text{sin. } \beta = \text{sin. } 9^\circ\,45' + \text{sin. } 9^\circ\,4' = \cdot 32693.$$

Hence the work transmitted per minute is

$$\frac{1}{l} \cdot \text{W}\,n \cdot \frac{2\,\pi\,r}{12 \times \cdot 32693} = \text{W}\,n \times 9{\cdot}289$$

and the transmitted H.P. is W n × ·000281.

In practice the driving part of the belt becomes tight, and the upper belt becomes slack, and the part of it between the dynamometer and the fly-wheel, on which there is very little tension, will drop very considerably. In some of the experiments, when there was a heavy load, the upper belt on leaving the dynamometer dropped quite to, and sometimes below, the horizontal. This would only occur with large loads, when there was very little tension on this part of the belt, and therefore the error from this cause would be small.

The speed was measured on the shaft pulley Q, and the moment of the balancing forces on the long arm of the lever equals W—the force which at a distance of one foot will balance the pull on the shorter arm of the lever.

Electrical Testing.—Two beams, A A (Fig. 5), about 10 feet long, were fixed, about 10 feet apart, on four posts. Across these were wound twenty-six coils of No. 16 galvanised iron wire. Each coil was about 60 feet long, had three full turns, and had a resistance (when cold) of about 1·5 ohms. The circuit containing the tangent galvanometer, N, had a resistance of 1·559 ohms. All the like ends were taken to one point, F, in the middle of a third beam, E F D, where connection was made with a short piece of rubber-covered, tarred, and taped wire which led to the resistance coils, R.

The other ends in two sets were taken to two mercury cups, E and D, connected by a short piece of cable, p. In the cup E was inserted one of the mains from the dynamo. In this way the current could be sent over from one to twenty-six parallel and nearly equal circuits. One of Helmholtz's double-coil tangent galvanometers, N, kindly lent by Elliot Bros., was placed in one of the circuits, a little removed from the rest. About 3·5 ampères gave a deflection of 45°, and in each experiment the number of circuits was arranged so as to give usually a deflection of from 35° to 55° on the galvanometer. In one or two cases higher deflections were unavoidable. With these currents, the wire, being freely exposed, only at times warmed to the touch. Immediately after an experiment the resistance of these wires was taken, and in no case was there an increase in resistance of more than 1 per cent.

There were three methods of measuring the current.

1. The wires on the frame being supposed to be of equal resistance, equal currents would flow in them, and the total current would be equal to the current in the galvanometer multiplied by the number of wires used.

$$C = n\,k \tan.\,\theta \quad \ldots \quad \ldots \quad \ldots \quad (a)$$

2. Supposing the wires not to be quite of equal resistance, then if $m =$ the resistance in parallel circuit of all the wires used, when cold, and g that of the galvanometer circuit, when cold, then

$$C = \frac{g}{m}\,k \tan.\,\theta \quad \ldots \quad \ldots \quad \ldots \quad (b)$$

3. The difference of potential at the terminals of the galvanometer circuit, being equal to the product of the current and the resistance of the wire at that instant, *i.e.*, when warm, or

$$E = C'\,g' = g'\,k \tan.\,\theta\,;$$

or, if m' is the resistance of all the wires, when warm,

$$E = C\,m' \quad \ldots \quad \ldots \quad \ldots \quad (c)$$

The difference of potential, E, was measured by a condenser and reflecting Thomson galvanometer. Even if the wires are not equal, (b) and (c) should give results which are very nearly equal.

German silver would have been preferred to galvanised iron

for the wire frame, if it had been available, because of its more even quality, higher specific resistance, and smaller change of resistance for change of temperature.

There could be very little leakage from one wire to another, for the extreme difference of potential at the ends of the wires was only about 6 volts.

The external resistances used were (1) coils of course German silver wire arranged in multiple circuit, and arranged so that a portion could be cut out or short-circuited, to give the required resistance. Or (2) boards wound longitudinally with fine iron wire and laid over one another in a large tub, the ends being connected and the bights brought to the edge of the tub clear of each other. Water was allowed to run into the tub, and the circulation kept up during the experiments.

One end of the short cable was connected to one end of these wires, and the return main to the dynamo hooked into the bight at such distance as was required to give the proper strength of current.

For higher resistances, the resistance of water between a piece of sheet-iron and an iron tube was employed.

The reflecting galvanometer, G (Fig. 5), had a resistance of about 11,000 ohms, and the condenser, C, had a capacity of half a microfarad. H is a shunt, P a reversing key, and T a discharge key. During the later experiments an Edison incandescent lamp, L, was used instead of an oil lamp for the reflecting galvanometer.

The instruments used for the potential and resistance measurements were kindly lent by Messrs. Clark, Muirhead, & Co.

Clark's standard cell (E.M.F. = 1·457 volts) was used for standard deflections, and compared with it a pint Grove's cell had an E.M.F. of 1·925 volts.

To determine the constant of the tangent galvanometer, a branch current from an "A" Gramme machine was sent through the thick German silver coil and the tangent galvanometer, and the difference of potentials at the terminals was taken by the condenser and compared with the standard cell, no shunt being used with the reflecting galvanometer in either case.

The mean of a great number of different determinations gave $k = 3{\cdot}469$ for the constant of the tangent galvanometer.

Three sets of readings of the dynamometer were taken—

(1) For friction of shafting alone;

(2) For shafting and dynamo running free;

(3) For shafting and dynamo with circuit closed.

The resistances of the dynamos and of the branch wires, and the deflections given by the standard cell, were taken during the two first experiments, and the electrical measurements with the dynamo running were made during the third experiment. A relation was established between the deflections given with the different shunts of the galvanometer, by means of 12 Leclanché cells. After the standard deflections were obtained, the difference of potential at the brushes of the commutator or at the terminals of the external circuit were obtained, and then the difference of potential at the points E and F, the terminals of the galvanometer circuit. After this the engine was indicated, and the speed of the dynamo found; then as soon as the current was broken and the dynamo stopped, the resistances of the armature and the field magnets were obtained.

Testing of Incandescent Lamps.—The lamps tested were—(1) Edison A 16-candle lamps; (2) Edison B 8-candle lamps; (3) Edison C 8-candle high-resistance lamps; (4) Swan 20-candle lamps; (5) British Lane-Fox high-resistance lamp; (6) Brush Lane-Fox lamp. Six of each of these kinds were tested. Besides these there were also tested—(7) Maxim lamps; (8) Swan small 10-candle lamps; (9) Swan high-resistance lamps; (10) Swan large 100-candle lamps. The efficiency of these lamps was tested, *i.e.*, the ratio of the quantity of light given by each lamp for each unit of electrical energy spent. The electro-motive force was measured in two ways—(1) by discharge from the condenser; (2) by a Thomson's voltmeter, and roughly by the number of Grove's cells employed. The current was measured, (1) by Thomson's current meter; (2) by a tangent galvanometer. The illuminating power of the lamp was measured on a Bunsen photometer, being compared with a lamp of about 17 candles, the accurate value being determined from time to time by careful comparison with a standard candle. The intensities were equalised in each case for red light and for green light (red and green glasses being employed), and the

intensity of white being deduced from the mixture of red and green in definite proportions, according to the scale given by Captain Abney.

Through the kindness of Professor Guthrie these experiments were carried out in the Physical Laboratory at South Kensington, by Mr. F. J. Sprague, Captain Abney, and myself, assisted occasionally by Colonel Festing and by Mr. Pierce. The same condenser and Thomson's reflecting galvanometer and the same Clark's standard cell were used as in the experiments with dynamo machines. The current was obtained from a battery of Grove's cells, and usually each lamp was tested in simple circuit, with battery powers varying from 20 to 50 Grove's cells, the small lamps from 10 to 25 Grove's cells, the Maxim to 70 cells, and Edison high-resistance lamps were tested to 80 and 85 Grove's cells. The object was to determine the law of efficiency of each kind of lamp for different currents, and for illuminating powers far beyond the usual illuminating powers of the lamps. It was thought that this severe test would give also some idea as to the lasting power of the lamp. The resistances of the lamps cold were measured on a Wheatstone bridge, and were as follows:—

No.	Edison. B.	Edison. A.	Edison. C.	Swan.	British.	Brush.	Maxim.
1	154·2	272·8	455·7	63·0	163·3	34·3	80·2
2	124·8	226·1	578·5	85·0	156·8	36·3	79·9
3	158·5	248·1	472·4	67·0	157·7	31·4	84.1
4	135·7	204·1	483·6	69·8	158·7	35·1	79·9
5	115·0	275·1	476·6	72·4	153·8	32·6	86·0
6	118·6	219·7	446·7	67·6	154·5	38·2	84·2

Besides these there were tested—Swan high-resistance lamps Nos. 13 and 14; Swan low-resistance 8-candle lamps, Nos. 15 and 16; Swan large 100-candle lamps, Nos. 17 and 18.

No. and Resistance.

13	14	15	16	17	18
194·0	186·2	26·4	20·0	47·8	53·8

An elaborate series of experiments was carried out by Mr. Sprague and myself to determine the constants of the Thomson current-meter and the Thomson voltmeter. The constant of the current-meter was found by comparison with a tangent galvanometer whose constant was carefully determined, and the value was less than the value given with the instrument, in the ratio of 15 to 16. The value of the constant for the current-meter was ·150—*i.e.*, the reading of the instrument when multiplied by this number gave the current in ampères. The value of the constant of the voltmeter was $\frac{7{\cdot}1 + {\cdot}18}{2} = \frac{7{\cdot}28}{2} = 3{\cdot}64$. The different graduations of the platform of the voltmeter were found to be consistent with one another.

The calculated results for some of the lamps tested are given in the following tables:—

As an illustration of the method of working out the characteristic curves for incandescent lamps, take the Edison "B" lamp, No. 4.

Resistance (cold) = 135·7 ohms.

For measurement of *current*—

$$\text{Constant of current-meter} = \frac{6{\cdot}54}{43{\cdot}6} = {\cdot}150 = \frac{3}{20}$$

Cells ...	20	25	30	35	40	45	50
Readings ...	2·9	4·0	4·8	5·95	6·9	7·95	8·7
Currents ...	·435	·6001	·7199	·8925	1·035	1·192	1·305

For difference of potential at the poles of the lamp—

(1) By Thomson's Voltmeter.

$$\text{Constant} = \frac{7{\cdot}1 + {\cdot}18}{2} = 3{\cdot}64 \ (\log.\ 3{\cdot}64 = 0{\cdot}5611).$$

Readings ...	10	12·4	14·7	17·0	19·1	21·3	23·5
	1·0000 0·5611	1·0934 0·5611	1·1673 0·5611	1·2304 0·5611	1·2810 0·5611	1·3284 0·5611	1·3711 0·5611
	1·5611	1·6545	1·7284	1·7915	1·8421	1·8895	1·9322
Volts	36·40	45·13	53·51	61·87	69·52	77·54	85·55

(2) By Condenser and Reflecting Galvanometer.

Value of shunt 10·27, deflection for standard cell = 37·5.

$$\text{Constant} = \frac{10{\cdot}27 \times 1{\cdot}457}{37{\cdot}5} = {\cdot}399 = {\cdot}4 - {\cdot}001$$

Readings ...	92	113·5	135·5	157	178	199	219·5
Volts	36·71	45·29	54·06	62·65	71·03	79·41	87·58
Mean volts ...	36·55	45·21	53·78	62·26	70·27	78·47	86·56
Mean watts...	15·84	27·13	38·69	55·56	72·71	93·60	112·93
Kilogram-mètres	1·614	2·765	3·943	5·660	7·407	9·536	11·50
Values of $\frac{E}{C}$	84·02	75·34	74·73	69·76	67·90	65·83	66·35
Candle power:							
(1) Red ...	·3412	1·843	5·302	13·760	27·21	46·58	79·00
(2) Green...	·2673	2·050	6·034	17·500	44·65	79·00	143·70
Mean corrected candle power	·3042	1·946	5·668	15·900	37·53	66·03	119·45

The characteristic curves, with ordinates representing candle-power and abscissæ representing kilogrammètres, are drawn for each lamp, and the mean of all the curves for the six Edison "B" lamps is the curve given in Fig. 7.

Take, as another instance, the Swan high-resistance lamps (No. 13). Resistance (cold), 194 ohms.

For measurement of *current*—

$$\text{Constant of current-meter} = \frac{6{\cdot}54}{43{\cdot}6} = {\cdot}150.$$

Cells	25	30	35	40	45	50
Readings	2·2	2·8	3·4	4·2	5·2	6·2
Currents	·33	·42	·51	·63	·78	·93

For difference of potential at the poles of the lamp—

(1) By Thomson's Voltmeter.

$$\text{Constant} = \frac{7{\cdot}1 + {\cdot}18}{2} = \frac{7{\cdot}28}{2} = 3{\cdot}64 \ (\log.\ 3{\cdot}64 = {\cdot}5611).$$

Readings	12·4	14.9	17·2	19·5	21·6	24·0
	1·0934 ·5611	1·1732 ·5611	1·2355 ·5611	1·2900 ·5611	1·3345 ·5611	1·3802 5·611
	1·6545	1·7343	1·7966	1·8511	1·8956	1·9413
Volts	45·13	54·24	62·61	70·98	78·63	87·36

(2) By Condenser and Reflecting Galvanometer.

Constant = ·399 = ·4 — ·001.

Readings	115	135	159	181·5	201·5	225
Volts	45·88	53·87	63·45	72·43	80·41	89·78
Mean volts	45·50	54·05	63·03	71·71	79·52	88·57
Watts	15·01	22·70	32·145	45·18	62·026	82·37
Kilogrammètres* ...	1·529	2·313	3·277	4·606	6·322	8·395
Values of $\frac{E}{C}$	137·9	128·7	123·6	113·9	102·0	95·21
Candle-power—						
(1) Red light ...	·6832	2·275	6·301	13·76	28·34	57·76
(2) Green light ...	·4638	2·275	8·806	19·72	44·65	82·75
Mean candle-power ...	·5735	2·275	7·544	16·74	36·50	70·26
Corrections for white light ...	...	...	·180	·50	1·36	2·10
Corrected candle-power	·5735	2·275	7·734	17·24	37·86	72·36

* = watts × ·10192.

Compare with this a low-resistance eight-candle Swan lamp (No. 15); resistance (cold) 26·4 ohms. For this lamp 10, 15, 20, and 25 cells were employed.

Cells	10	15	20	25
Readings	8·3	12·3	16·5	20·5
Currents	1·245	1·845	2·475	3·075
Differences of potential—				
(1) By voltmeter	16·744	24·752	32·032	38·584
(2) By condenser method ...	16·359	24·339	31·920	38·304
Mean volts	16·551	24·545	31·976	38·444
Watts (E C)	20·606	45·286	79·140	118·215
Kilogrammètres	2·1	4·62	8·072	12·057
Values of $\frac{E}{C}$ (in ohms)	13·3	13·3	12·9	12·5
Candle-power—				
(1) Red light	·5888	7·776	36·24	82·75
(2) Green light	·4264	11·260	63·10	149·40
Mean candle-power	·5076	9·518	49·67	116·07
Corrections	...	·300	2·70	7·40
Corrected candle-power... ...	·5076	9·818	52·37	123·47

The characteristic curves for each lamp having been drawn, the curves of lamps of the same kind were grouped together, and the mean curve of each group is the characteristic curve for that group, and is represented in Fig. 7 for the series of lamps tested.

Relation between work expended and candle-power in Maxim incandescent lamps:—

For measurement of current—

Cells	20	25	30	35	40	45	50	60	65	70
Readings ...	4·9	·2	7·6	9·0	10·45	11·9	13·4	16·5	17·8	18·5
Current in ampères	·735	·930	1·140	1·350	1·567	1·785	2·010	2·475	2·670	2·775

For difference of potential at the poles of the lamp—

(1) By Thomson's Voltmeter.

Readings ...	9·75	11·9	14·1	16·2	18·45	20·5	22·5	26·4	28·1	30·05
Volts	35·49	43·31	51·33	58·94	66·97	74·63	81·91	96·10	102·3	109·3

(2) By discharge from a Condenser.

Readings ...	89	110	131	151·25	171·75	191·25	211	248	266	284
Volts	35·511	43·89	52·269	60·349	68·329	76·308	84·189	98·948	105·735	112·917
Mean volts ...	35·50	43·60	51·80	59·65	67·65	75·47	83·05	97·53	104·02	111·10
Watts... ...	26·09	40·55	59·05	80·52	106·0	134·7	166·9	241·4	277·7	308·3
Kilogram-metres	2·659	4·131	6·016	8·204	10·80	13·73	17·01	24·60	28·30	31·42
Values of $\frac{E}{C}$...	48·29	46·88	45·43	44·19	43·16	42·29	41·32	39·41	38·95	40·03

Measurement of candle-power—

Red light ...	·4264	1·945	6·301	15·52	33·37	60·34	95·23	207·6	263·1	388·9
Green light ...	·2954	1·945	7·152	18·95	41·90	79·00	149·4	318·0	388·9	482·3
Mean	·3609	1·945	6·726	17·23	37·63	69·67	122·31	262·8	326·0	435·6
Correction ...	—·01		+·06	+·25	+·61	+1·36	4·50	9·0	10·5	18·5
Corrected Candle power	·3509	1·945	6·786	17·48	38·24	71·03	126·8	271·8	336·5	454·1

From the values of $\frac{E}{C}$ for Maxim lamps it appears that up to a little beyond 300 candle-power the resistance continues to diminish, but that the resistance for higher illuminations again increases, showing that from about 300 candle-power the breaking down of the lamp has begun, and the lamps break after attaining an illuminating power of about 450 candles.

The characteristic curve for Maxim lamps is not continued to the extreme value in Fig. 7, but is of the same general character up to the point where it begins to break down. At 32 candle-power there is a yield of 240 candles per H.P., as compared with 342 candles for Edison lamps, and 415 candles for Swan lamps. At 40 candle-power there is a yield of 280 per H.P., as compared with 400 candles for Edison lamps, and 500 candles for Swan lamps. At 140 candle-power the yield is 600 candles per H.P.—*i.e.*, the economical efficiency is the same as the economical efficiency of Swan high-resistance lamps at 60 candle-power for each lamp.

The efficiency of the Maxim lamp becomes about 920 candles per H.P. when the lamp is giving an illumination of about 300 candle-power, and so is comparable with the efficiency obtained in lighting by the electric arc.

With the Maxim lamp the illumination was not equal in all

directions on account of the flatness of the filament. At half a right angle from its position of brightest illumination, the candle power was reduced about 7 per cent., and near the plane of the filament the illumination was only about 75 per cent. of the brightest illumination.

Professor W. E. AYRTON: I have the greatest pleasure in proposing a vote of thanks to our new President for his extremely interesting address—an address which, unlike many speeches we have heard, became more and more interesting as it went further on. The great point of interest in the address has obviously been the information which Professor Adams has given us about the tests that he made as one of the jurors of the Crystal Palace Electrical Exhibition. I have been afraid for a very long while that the Report of the Jury would never come to light; and I have used my efforts on various occasions to induce Professor Adams to allow our Journal to have the benefit of the results. I am now happy to be able to say that I trust that the Journal will have that benefit.

Attention was drawn in the address to the very important point of the difference between the electric efficiency of a dynamo machine and its commercial efficiency. Through the last year or two I have constantly heard it said, What is the use of troubling about experiments with dynamo machines? they are all good, and give 90 or 91 per cent.,—possibly it being quite forgotten that such numbers referred to the total electric efficiency, and not that which Professor Adams has rightly called the commercial efficiency. Were it not an inaugural address, which does not admit of discussion, I should like to ask Professor Adams whether in the resistance of the armature he has taken into account the increase of resistance arising from self-induction. This of course would not affect the commercial efficiency (which is the difference of potential at the terminals multiplied by the current in the outside circuit), but it would affect the total electric efficiency, because the resistance of the armature when rotating is larger than the resistance measured when at rest by means of the Wheatstone bridge.

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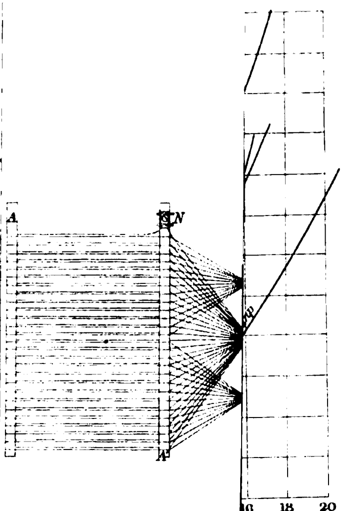
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The remarks made by Professor Adams about gas-engines are of immense importance, because it appears to me, and I am sure it must appear to all present, that the gas-engine has an extremely wide future before it, partly on account of the high efficiency that it does and must possess, and partly on account of the great ease with which it may be worked. The great objections that have hitherto existed to the employment of a gas-engine have been the difficulty in starting, its unsteadiness, and the cost of gas. The first difficulty has been overcome in the Clerk engine by the employment of an automatic starting gear worked by air or gas compressed by the engine just before it stopped. The unsteadiness is being very rapidly got over, and the governor of a gas-engine specially made for Professor Perry and myself during the last month, by Messrs. Crossley, is extremely perfect. The specification for the engine was a very tough one, and required that it should be a six horse-power engine, and should be able to change the load from one to six horse-power, with not more than two revolutions per minute. And I am happy to say that Messrs. Crossley have exercised their ingenuity, and have produced a governor which, if not quite fulfilling this somewhat stringent specification, a result hardly to be expected, is nevertheless very satisfactory. The cost of gas is also being very rapidly got over by abandoning illuminating gas, such as we ordinarily have, and using some cheap form of heat-giving gas. I am told that experiments made at Messrs. Crossley's works during some months upon a large gas-engine, used for driving the whole of their works, employing Dowson's gas, have led to the result that power as steady as that of steam can be produced with a consumption of 1·1 lbs. of coal per horse-power per hour—a result comparing most favourably with those obtained with the largest marine condensing steam-engines. The gas-engine, it seems to me, has a very large future, and indeed will vie with small motors for general distribution of power throughout large towns. It is not very clear at this moment which will gain the day—whether we shall have motors put in every house after electric mains are laid down, or whether we shall have very small gas-engines, of considerable economy and great ease in starting and stopping, worked by heat-giving gas supplied

from either the present mains or from other gas mains that will be laid down.

Time is too late to enter into any of the various theoretical or practical points suggested by the paper; therefore I will propose that the thanks of the Society are due to the President for the interesting and valuable address delivered by him this evening, which it is requested that he will allow to be printed and published in the Journal of the Society.

Mr. C. E. SPAGNOLETTI : I have very great pleasure, indeed, in seconding the vote of thanks proposed to Professor Adams for his instructive and interesting address. An address of this kind means the occupation of considerable time and careful attention. He has entertained and instructed us so much, that I am sure we all feel very much indebted to him for the trouble he has taken. He has touched on many interesting points, and given us a mass of information to consider, and his address will form a valuable addition to our proceedings. I beg to second Professor Ayrton's proposition, and do so with very great pleasure.

Mr. WILLOUGHBY SMITH : It has been proposed by Professor Ayrton, and seconded by Mr. Spagnoletti, that we give our President our hearty thanks for the very able address which he has given us this evening. I do not think it requires any further words from me, for I am sure the feeling is unanimous. The vote was cordially expressed.

The PRESIDENT : Gentlemen,—I am very much obliged to you for the kind way in which you have listened to me this evening. I am afraid that at the beginning of the year I have given you a severe trial; I have tried your patience for a considerable time, and I will not now encroach any further upon it. I will only say that during the coming year I shall exert myself, to the best of my ability, to further the objects of the Society. But the success of the Society cannot depend upon your President: it must depend greatly upon the Council, who have always devoted themselves to its welfare; but I would say, also, that it must depend very greatly upon the other members of the Society, from whom the Council hope to receive many excellent papers. I trust that next year, when we get our report, we may find that we have

had so many valuable papers sent in by the members of the Society, that the Committee sitting upon those papers shall have very great difficulty in deciding to whom they shall award the prizes, but that they may be able to award all the prizes after careful consideration, and not only give the prizes, but make honourable mention of several who have been very nearly winning them. I trust that our Society will be as successful during the coming year as heretofore, and I am very much obliged to you for the kind way in which you have received my address.

A ballot took place, at which the following were elected:—

As Foreign Members:

Christian Jensen. | Maurice Simon.

As Member:

Captain H. L. Wells, R.E.

As Associates:

George Leonard Addenbrooke.	P. J. Kelly.
J. Ardron.	John A. Kingdon, B.A.
Henry Leslie Ashmore.	H. W. Kolle.
Morgan Mark Bevan.	Frank Lumley.
Walter Carey.	J. P. MacGregor.
C. J. Cartwright.	H. Nalder.
Charles Ashley Carus-Wilson.	William Herbert Peacock.
John H. Greenhill.	Alfred Perkins.
Richard Robert Harper.	Oliver William Smith.
Charles Hortsek.	O. W. Stevens.
Albert Hoster.	John William Ullett.
Léon Husson.	Norman Ward.

Otway Edward Woodhouse.

As Students:

Oldbury Burne. | Frank Mercer.

Alfred Leonard Stocken.

The meeting then adjourned until Thursday, January 31st, 1884.

The One Hundred and Twenty-ninth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 31st, 1884—Professor W. Grylls Adams, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates were announced and suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

G. James Morrison, C.E.

From the class of Students to that of Associates—

William John Hancock, Jun.

Donations to the Library were announced as having been received from Lord Rayleigh, the Royal Institution, the Institution of Mechanical Engineers, Mr. E. Gray, Sir Charles Bright, and Lady Siemens, to all of whom the thanks of the Society were duly accorded.

The President: I should like to draw the special attention of the members to the collection of books which has been presented to the Society by Lady Siemens, and to say that it formed a valuable portion of the library of the late Sir William Siemens. It consists of 320 volumes, including the *Minutes of Proceedings of the Institution of Civil Engineers* (65 vols.), the *Proceedings of the Institution of Mechanical Engineers*, the *Transactions of the Institution of Naval Architects*, 31 volumes of the *Fortschritte der Physik*, 22 volumes of the *Philosophical Transactions*, 26 volumes of the *Proceedings of the Royal Society*, and many other valuable works. This forms a very valuable addition to the Society's library, and the Council have thought it right to mark their sense of its importance by passing a special resolution on the subject, giving our thanks to Lady Siemens, to which I will ask the approval of the meeting. The terms of the resolution are these: "The President, Council, and Members of this Society desire to record the deep sense they entertain of the feeling

which prompted Lady Siemens to present to the Society so important an addition to their Library; and to assure her that the value of her generous gift is greatly enhanced in their estimation by the circumstance of the books having formed a portion of the personal collection of their much esteemed and respected Past-President."

Mr. WILLOUGHBY SMITH seconded the resolution, which was put to the meeting and carried unanimously.

The following paper was then read :—

ON A SYSTEM OF ELECTRIC FIRE-ALARMS.

By E. B. BRIGHT, C.E., Member of Council.

Seven years ago a paper was read before the Society on the subject of the street electric-calls established in certain foreign cities. At that time there were no fire-alarm call-posts in use in London, but during the last three years an extensive system has been constructed, both in the city and suburbs.

The application of electric fire-alarms to the protection of life and property deserves a great deal more attention than has hitherto been accorded to it in this country.

In the United States and in many countries of Europe such appliances have been very extensively adopted for many years; but in the United Kingdom, though the lives and property at stake are certainly not less important, very little has been done in this direction.

Lately, however, in London, under the auspices of the Metropolitan Board of Works and Capt. Shaw, C.B., the chief officer of the Fire Brigade, an extensive system of electrical call-points has been carried out by the telegraph authorities of the Post Office.

I regard simplicity and reliability as the great essentials of fire-alarms.

In those cities and towns of Europe and America where street fire alarm-calls have so far been introduced, various modifications of clock-work have formed the leading characteristic of the apparatus; not only costing a good deal, but, from its very nature, subject to get out of order from rust, wear and tear, and other causes.

In those which form the subject of my paper, the locality of the fire, or rather call-post from which the summons to the engines is given, is shown by a few yards of resistance wire in the post or call-box.

I may here remark that the system of fire-alarm apparatus I am about to describe is based upon the principle of the system patented by Sir Charles Bright and myself in October, 1852, for ascertaining the locality of faults in telegraph conductors by means of a set of varying resistance coils, which were brought successively into a local balancing-circuit in connection with a differential galvanometer, the other side of which was joined to the faulty line wire; this being continued until the balance of the needle was arrived at, and the distance to the point of fault thus known.

This invention, which has since been so greatly used both in connection with cables and land lines, is shown in the first diagram, where a fault is indicated in the conductor; and at the distant station a battery is shown with one pole connected to earth and the other pole with a differential galvanometer, and thence with the faulty conductor on one side and with a box of coils of consecutively increasing resistance on the other, which are introduced into the local balancing circuit by turning the handle, connected to earth, of the commutator, until a balance is effected and the approximate distance of the fault thus ascertained.

This fault-localising invention was described by Dr. Lardner in 1854, in his "Museum of Science":—"One of the objections against the underground system of conducting wires was, that to ascertain the position of faults required a tedious process of trial to be made from one testing-post to another, over an indefinite extent of the line. A remedy for this serious inconvenience, and a ready and certain method of ascertaining the exact place of such fault without leaving the station, has been invented and patented by Messrs. Bright in 1852." Then follows a description of the apparatus, similar to that shown in the first of my diagrams.

It will be seen from the second diagram, which represents the line connections and station apparatus of a London fire-alarm circuit, that instead of localising a fault in connection with earth

on a telegraph conductor, where the resistance of the fault itself will have to be considered and allowed for, that in this fire-alarm apparatus the resistance of the coil of wire brought into circuit by a "call" at a post is practically invariable, corresponding with the coil of wire put in by the movement of the handle of the brigade station commutator, to balance it. The form of post is shown in Diagram 3. In each call-post there is a coil of wire of definite resistance which is introduced into the line circuit by simply withdrawing a short-circuiting handle or plug.

The line wire is balanced at the Fire Brigade station by an equal resistance, and, instantly the additional coil is introduced at the street post whence the alarm is given, the galvanometer relay is deflected and rings the alarm. The turning of the station indicator, which consecutively inserts additional resistance coils, restores the balance as soon as the particular resistance represented by the alarm-post is added, and thus indicates the particular post whence the call emanates.

This turning of the indicator may either be effected by hand, or automatically by a simple step-by-step arrangement in which the pointer stops on the dial on the balance being restored.

In the Metropolitan fire-alarm system, Captain Shaw has preferred to employ constant currents; and an advantage of the line wire being balanced as I have described consists in the fact that the wires must be kept in good order, as any short-circuiting or bad connection would at once disturb the balance, and, by sounding the alarm, call attention to the wire or connections being out of order. In case of a short-circuit the needle of the galvanometer relay is necessarily deflected to the opposite side to that resulting from a fire call.

It was deemed desirable that an acknowledgment should be given from the brigade station to the person giving the call, and with this object the resistance wire is wound upon an iron core, and thus converted into an electro-magnet in proximity to an armature, with a light red disc at its end, as shown in Diagram No. 4.

On the current passing through the coil when a call is given, the armature is attracted, and shows the red disc before a little hole in the front of the box.

The acknowledgment is given from the engine station by making and breaking the circuit with an ordinary key, thus occasioning the disc at the alarm-post to wave to and fro.

Where it is not necessary to provide for an answer being given, as in the Museum at South Kensington, the apparatus is further simplified, only requiring a coil of resistance wire and a contact plug, as shown in the apparatus I now exhibit.

In the street call-posts glasses are fixed in the doors, in order that, by breaking the glass and pulling the handle inside, any passer-by can give the alarm on seeing a fire; and, in order to facilitate this, I have the glasses so fitted that they are held in position by three iron nuts: thus, on receiving a blow, the glass falls into the post in three pieces, leaving the space clear for the hand.

Keys are issued to the police and fire-escape men by which they can open the doors of the posts and give an alarm, without breaking the glass. I suggested to the Metropolitan Board that glasses should not be used, but a large number of numbered keys issued to the leading occupiers, as well as to the police; the key to break contact and give the alarm-call on being inserted, and fitted with a collar to be held by a spring on insertion, to prevent its withdrawal until the post was opened by the fire brigade staff. If a false call was then given, the person giving it could probably be then traced by the number on the key.

In practice it has not been found necessary to provide for an indication of more than one call at a time, but I arranged a slight modification of the station apparatus to enable this to be done if desired. After turning to one call, the resistance it represents is then (by moving a plug) left in the station balancing-circuit, and the commutator handle is moved back to zero. The apparatus is then ready for another call; and, as the resistances are all different, it is obvious that this process may be repeated for as many calls as there are call-points connected to the station commutator, and yet that the locality of each call will be separately indicated.

By also proportioning the resistances, it is readily arranged that no two calls, if given at the same moment, will be equal to any other single call.

As a proof of the great advantage of the street-call system, I may mention that, when the two first circuits on my system were put up in August, 1880, in the City and Southwark, in connection with only fifteen call-points, I obtained particulars showing that in the first ten months of working there were no less than fifty calls by this means to fires in this comparatively limited area. It is hardly necessary to dilate on the vital importance of the engines being thus expedited to get on the spot at the earliest possible moment, so that a small fire may be checked at the outset, and prevented from becoming a large one.

The following is a list of the ten months' calls to fires on the two circuits referred to:—

Locality of Fire.		Locality of Fire.	
1880.		1881.	
Aug. 28.	Newgate Street.	Jan. 28.	50, Southwark Street.
„ „	Daniel Lambert Tavern, Ludgate Hill.	Feb. 2.	Aldersgate Street.
Sept. 7.	Dowgate Dock.	„ 5.	2, Queen Street.
„ 9.	Aldersgate Street.	„ 15.	72, Blackfriars Road.
„ 11.	Old Jewry.	„ 19.	Wood Street.
„ 13.	Thames Street.	„ 21.	Lawrence Lane.
„ 14.	Basinghall Street.	„ 24.	Winchester Wharf.
„ 25.	Botolph Lane.	„ 26.	Cannon Street.
„ 30.	New Broad Street.	Mar. 3.	Paternoster Square.
Oct. 9.	Pocock Street, Borough.	„ 15.	do. do.
„ „	St. Mary-at-Hill.	„ 30.	George Yd., Aldermanbury.
„ 11.	211, Borough High Street.	April 5.	Prince's Street.
Nov. 11.	Gutter Lane.	„ 10.	Aldersgate Street.
„ 21.	21, Borough High Street.	„ 20.	Cannon Street.
„ 27.	Newgate Street.	„ 21.	Lambeth Hill.
„ „	Sherbourne Lane.	„ 29.	Southwark Bridge Road (Cork Merchant).
Dec. 12.	Queen Victoria Street.	May 2.	Cannon Street.
„ 18.	Fore Street, Cripplegate.	„ 6.	Gresham Street.
„ 20.	Noble Street.	„ 7.	Queen Victoria Street.
„ 22.	St. Swithin's Lane.	„ 11.	Southwark Street (Millington, Paper Merchants).
„ 24.	Borough High Street.	June 1.	Monkwell Street.
„ 29.	Great Charlotte Street.	„ 8.	Waterloo Road.
1881.		„ 13.	Borough High Street.
Jan. 1.	Newgate Street.	„ 16.	Newington Causeway.
„ 13.	Poultry.	July 2.	Blackfriars Road.
„ 26.	Well Street.		

The various circuits upon which the system of call-points I have just described is fixed at present, are shown on the map of London (Diagram 5), and are as follow:—

Circuit.		Mileage. M.	Yds.		No. of Call-points.
City (Watling Street) ...	...	2	650	......	10
Southwark	...	2	920	......	6
Hoxton	...	3	700	......	4
Knightsbridge	...	3	370	......	7
St. Luke's	...	3	1,080	......	9
Lambeth	...	3	830	......	8
Hammersmith	...	7	—	......	9
Fulham	...	3	290	......	5
Highgate	...	5	1,610	......	6
Hampstead	...	4	120	......	4
Notting Hill	...	7	835	......	9
Hackney	...	6	50	......	6
Islington	...	5	85	......	6
St. John's Wood	...	5	1,400	......	8
Bow	...	3	60	......	3
St. Pancras	...	4	980	......	5
Holloway	...	7	510	......	7
Stoke Newington ...	...	4	20	......	3
Ladbroke Grove	...	4	880	......	8
Crystal Palace	...		1,320	......	8
South Kensington Museum	...	1	440	......	9
TOTAL	...	88	830		140

The engine stations of the Fire Brigade are all connected together by telegraph, so as to supplement with additional aid when required.

While on the subject of the considerable extension of street fire call-points that has been made at Captain Shaw's instance by the Metropolitan Board of Works in conjunction with the postal telegraph authorities, who arrange and carry out the works and fit up the apparatus, I am sorry to have to refer to the hindrance put in the way of these life and property saving appliances by certain suburban highway boards, notably that of Wandsworth, who control the large district about Putney, Wandsworth, and that neighbourhood; where, owing to their interference, no call-posts are yet erected. Their contention is that from the point of

possible breakage of overhead wires somebody may be injured; but I am not aware in all my telegraphic experience of over thirty years of any one being killed, or even seriously injured, by the breakage of wires erected on posts on roads. The cost of an underground wire by itself, entailing the opening of a trench and laying a pipe with a gutta percha wire, and its additional protection, is on the average ten times that of an overhead wire, and obviously would make the establishment of considerable lengths of fire-alarm wires, however beneficial and life saving to the community, beyond the reasonable cost that ought to be entailed in carrying them out.

In the late gale, I have read during the last few days very many accounts of the loss of life and property from chimneys, hoardings, walls, and roofs, but not one case as due to telegraph or telephone wires.

The employment of telephones has been suggested in connection with street fire-alarms; but there would be some difficulty in using them in crowded and noisy streets, and many of the ordinary public would be puzzled how to manipulate them. The strength of the battery currents that are constantly passing would also be in the way.

In my system the wire can, however, be employed for speaking purposes when no fire-call is being given, viz., by using an ordinary key to break the circuit at any point or points on the line; and this might be combined, if desired, with an ordinary recording instrument.

As regards the street call-posts, it should, however, be borne in mind that their introduction, advantageous though it is, only meets the evil half-way; for, as a rule, the call is not given to the engine station till some one has observed flames at a window, when the fire has made some head, and perhaps a long time after it has been smouldering and producing warmth enough about the ceiling of the room (to which the heated air, of course, at once rises) to give the alarm, by means of self-acting heat-detectors and appliances such as I will now describe.

House Alarms—Self-acting.

There are various contrivances by which an undue or abnormal

increase of temperature in a room may be made to give an alarm by electricity—thus, the rising of mercury in a tube, or the melting of easily fusible metals; but I think the cheapest and most convenient is a bi-metallic spring, such as shown in the Diagram No. 6. By making it of brass on one side and, say, steel or platinum on the other, the difference of expansion of the metals causes the spring to move until it comes into contact with a screw terminal which can be adjusted to the desired temperature. This is shown by the following experiment, which, to make apparent to all present, I exhibit by the magnified shadow of the spring affected, which is thrown upon a screen by the oxy-hydrogen light. The "localisers" I employ for self-acting heat-detectors (as shown in Diagram No. 7) differ greatly from those used in the United States, inasmuch as, instead of being somewhat complicated and expensive clock-work arrangements, such as I now produce, mine consist of a few yards of resistance wire in a small case of wood as now shown, the space of a cubic inch sufficing, which may be built into the wall, and will probably last as long as the building itself.

As the heat-detectors may be set to give warning at any temperature exceeding that of the normal state of the air in a building, they can also be employed to indicate the commencement of any heating in heaps of corn, jute, etc., either when on board ship or stored in warehouses, thus calling attention before actual harm is done or spontaneous combustion sets in.

In the same way, the heating of coal on board ship can be at once detected either in hold or bunkers. We all know this is a prolific cause of fires at sea, which I regard as the most terrible of calamities that can arise, involving, as they often do, the loss of the whole or part of those on board.

Many cases illustrating the advantage of the self-acting electric alarm have come to my knowledge; and one of the most striking instances occurred in New York, where a building in Greene Street was so protected, with its wire in direct connection with the fire brigade station. A fire started, and the heat-detectors gave the signal. Before it was known to any one about, the engine was at the door. The watchman in the building

refused admission, saying there was no fire, but an entrance was forced and the fire quickly extinguished on the fourth floor, where it was commencing, and whence the self-acting alarm had been given.

In one of the places where the self-acting arrangement of my apparatus has been established,—the North Shore Mills at Liverpool,—where more than 200 heat-detectors have been fixed through the mill rooms for several years, no less than three fires have been nipped in the bud by the instant notice given.

I may mention that when the system was exhibited at the International Electrical Exhibition at Paris in 1881 it gained the only gold medal awarded to fire-alarm apparatus, and a gold medal was also given at the English Exhibition of 1882.

Although great expenditure and attention have been applied to perfecting the best mechanical means of saving life at fires and for extinguishing them, scarcely anything has yet been done in this country to provide for instantaneous warning of the very beginning or smouldering of fires, even before an outbreak, both to the inmates of the building and to the brigade stations, by means of self-acting electrical alarms, set in operation by undue heat in any part of a building.

The great benefit to the community resulting from such appliances is, however, well recognised in the United States, where during the last ten years they have been largely introduced in New York, Boston, and other cities. This may be said to have resulted from returns made some years ago by the New York Fire Patrol Committee, which demonstrated that, apart from the number of lives saved, the result of nearly three years' working of the self-acting alarms had been a reduction of three-fourths of the loss to property where fires started in premises so protected.

The following are the figures:—

Insurance losses paid on fires occurring in buildings fitted with the self-acting electric fire-alarm, all of which were detected and reported automatically, amounted to $396,908.

The amount insured on these protected buildings and their contents amounted to $7,031,000.

The proportion of loss to insurance where the self-acting fire-alarm was fixed was therefore only about $5\frac{1}{2}$ per cent.

As a contrast to this, the insurance losses on risks of the same class in buildings not fitted with the self-acting fire-alarm amounted to no less than $4,530,928, as against an insurance of $18,990,901, or a loss to insurance of nearly 24 per cent. This shows that fires of which warning was given on the first development of undue heat in any part of a building fitted with self-acting electric heat-detectors were extinguished promptly before they had caused much damage, while those not so reported reached a more dangerous state before they were controlled.

It must be borne in mind that the means for extinguishing were identically the same in both cases, and that the difference of about 75 per cent. in favour of the electrically-protected premises was solely due to the promptitude with which the alarm was given by the self-acting heat-detectors.

Turning the above figures into English money, they show the actual saving in connection with the buildings included in this return to have been £258,000; while if the system had been applied to the other buildings which took fire and were not provided with the self-acting heat-detectors, the additional saving to the insurance offices would have amounted to £692,000.

The effect of this was that the insurance companies issued advertisements from time to time on that side of the Atlantic, and our companies insuring in the United States had to follow suit. The Commercial Union Assurance Company, for instance, offered 25 per cent. abatement in premiums on premises fitted with the self-acting alarms signalling undue heat, and the Scottish Commercial Company 20 per cent.

This eventually led to an arrangement as to premiums being generally reduced 10 per cent. on such protected buildings by all the leading American insurance companies, as well as by the principal English companies as regards their business in the States.

The following extracts show that the self-acting alarms have commended themselves to the Government of the United States, as regards their introduction into the public offices:—

From the Report of a Commission to examine into the Security of the Public Buildings in the City of Washington against Fire, appointed by President Hayes on 27th September, 1877.

In addition to urging the provision of ample apparatus for extinguishing fires, the Commission expressly recommended "the use in all rooms for storage purposes, and in those which have not to be visited frequently, of some electrical communicator, to be placed so that unusual degrees of heat may be signalled to the watchman; thus making known the fact of the presence of fire, should any occur in these apartments."

This was followed up by a measure passed by Congress, entitled "A Bill to aid in the protection of the public records and property against loss and damage by fire," in which it was enacted "that in all buildings containing public property or public records of the Government of the United States, the head of the department having control of such buildings is hereby authorised to put up and use an automatic signal-telegraph of such improved kind and description, adopted and now in public use, as is fitted and adapted to transmit signals of fire by means of unusual degree of heat."

The fire insurance companies in the United Kingdom receive premiums to the amount of more than twelve millions per annum, of which, on the average, they repay to insurers for losses by fire about fifty per cent., or six millions per annum. I get these figures from the "Insurance Cyclopædia."

These companies make considerable reductions where extinguishing appliances, such as hydrants, extincteurs, and water buckets are kept on the premises insured; but, so far, the Tariff Committee of the United Tariff Companies have persistently declined to make any concession in rates in connection with self-acting electric fire-alarms, which afford the means of bringing hydrants and extincteurs to bear on a fire at the first start, when they may be used with real effect.

I urge attention to this, because a reduction of insurance premiums would at once give a great impetus to the adoption of the self-acting alarms; for owners and occupiers would find, from the comparatively inexpensive character of such appliances, the

abatement by the insurance companies would in a year or two cover the cost of its introduction, while the companies would certainly have the opportunity of gaining considerably, if anything like the results experienced in New York, a saving of 75 per cent. in losses, was effected.

The use of such warning apparatus is of even more importance in country mansions, where there are no fire brigades and probably no passers-by at night, than in towns. They are mostly supplied with some sort of extinguishing appliances, but, when all are asleep, it is the little electric fire-alarm that can alone rouse them promptly, and enable the fire to be nipped in the bud. Witness the sad list of some of the noblest mansions in this country destroyed during the last few years, with their invaluable and irreplaceable works of art and historical interest.

I may instance Inverary Castle, the seat of the Duke of Argyll; Warwick Castle; Cortachy Castle; The Pynes, belonging to Sir Stafford Northcote; Marden House; Park Hall, Lord Yarmouth's seat, and many other recent cases, a large proportion of which would probably have been saved had self-acting alarms been fixed, costing a few shillings a room, and ringing to one or more of the bedrooms.

It appears almost inconceivable that such apathy should exist on the subject, and especially when we bear in mind the disproportionately greater sums spent in connection with the drainage, ventilation, water-supply, and lighting of such mansions, and that the expenditure on extinguishing appliances, such as hydrants, in many cases is ten times as much as the alarms would entail. The insurance companies could, if they cared to do so, give the greatest impulse to the extension of self-acting fire-alarm apparatus in this country by the reduction of their premiums in relation to premises where they were fitted and properly arranged. They have already made considerable allowance in connection with buildings in which provision is made for hydrants and the use of extincteurs and hand-pumps, having been compelled some years ago to do so by the competition of some of the northern mutual companies.

Now, the self-acting fire-alarm is the precursor to early and

useful application of the extinguishing apparatus. Hydrants or buckets are of little use if a fire is making head for want of that electric warning which may so instantly and cheaply be given where a number of these small self-acting tell-tales are ranged through a building, and, set at, say, 110°, call attention to the particular room in which they are placed directly even the smouldering of a fire commences, and long before any sign of flames may appear at a window. Yet the insurance companies, as a whole, have systematically ignored this, as I have found from experience.

The chief of an important company several years ago proposed that they should introduce the self-acting electric alarm on their own account into the warehouses they insured; but on reflection they decided against it, because a large proportion of the goods in the stores, belonging to various merchants, were insured by other companies, and they were not therefore inclined to adopt it when their rivals might reap the greater benefit.

As a reason for turning a deaf ear to the alarm, the manager of one of the largest insuring societies frankly said that the general use of such appliances might militate against their business, inasmuch as they found that a large fire now and then actually benefited them by bringing a shoal of new insurers.

The companies also urge that the rates of premium are already too low; but I may remark that they omit from consideration that if the loss from fires, by the use of any appliance, is reduced by any given percentage, they are not supposed to be expected to allow more than a part of the saving in the premiums charged to their customers.

Perhaps it is not to be expected that the fire insurance companies will voluntarily abate their premiums to aid the introduction of appliances which tend to prevent great fires by giving such instantaneous warnings as may stay their extension; but it should be borne in mind that the saving of life is a very essential point in dealing with the subject, and that if insurance companies do not come forward, then the use of such life-saving and property-saving apparatus should be enjoined by Act of Parliament, just as the building of solid walls and the supply of

fresh water and standard gas are already, with many other matters, such as sewage works, etc., inculcated by national authority. When, in connection with any important building, we consider the cost of the arrangements for drainage, lighting, water-supply, and probably hydrants or other fire-extinguishing appliances, it must seem almost marvellous that the expense of a few shillings a room for self-acting heat-detectors and electric fire-alarm apparatus should not be included, considering its small percentage to the other charges. We may see it compulsory, and it should be remembered that, in buildings where electric bells are used, the fire-alarms can be added by simply extending the bell wires up to the ceiling of each room.

In conclusion, I would strongly impress upon all who may hear or read my remarks on this subject, that there are three degrees of excellence in connection with the application of electric fire-alarms.

The preliminary one is the introduction of call-posts in the streets.

The next is the introduction of self-acting heat-detectors throughout buildings, so as to warn the inmates of the commencement of undue heat in any room, and, by sounding a gong or other signal at the door or outside the premises, to call the attention of passers-by or the police.

And the third, and most important, is the direct connection of these internal self-acting appliances with the nearest fire-engine station.

Shakspeare put the matter well when he wrote :

> "A little fire is quickly trodden out ;
> Which, being suffer'd, rivers cannot quench."

The PRESIDENT : Before asking you to give your thanks to the author for his paper, I would invite the members present to discuss it. From the manner in which it has been received it is quite evident that the subject is an interesting one, and there can be no question that it is of vast importance. I have no doubt there are many present who would like to join in the discussion, and, as time permits, I now invite them to do so.


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Mr. C. E. SPAGNOLETTI: Mr. President,—The subject of Mr. Edward Bright's paper is one of peculiar interest to myself, because I have given much attention to fire-alarms for many years past, and all that he has said I quite endorse. There is every necessity for the protection of property in every conceivable way. The system of self-indicating heat-alarms, or thermostats I suppose they may be called, is a most excellent system, and I am very glad to see the delicate way in which it works, from the illustration we have just had. I have been trying a somewhat similar system for years past in our railway signal lamps, to indicate to the signalman, where they cannot see the lamps, whether they are in, out, or burning badly. The system works exceedingly well and satisfactorily: there are some 500 lamps fitted with the apparatus, and they certainly do indicate in a most admirable manner. When the system was tried with gas-signal lamps it was not so successful, because of the variations which frequently take place in the pressure of gas, it being sometimes higher and sometimes lower in the mains; and it was not until gas-governors were introduced for the burners that success in that direction was achieved, since which everything has gone on satisfactorily. Ordinary oil lamps do not work well, because the wicks become carbonised, and from other obvious causes, but with petroleum or other mineral oil satisfactory results are obtained and continue.

In connection with the thermostats for buildings, which Mr. Bright has shown us, I am reminded of a system which I saw ome time ago, and which came from the other side of the tlantic, which might, I think, be incorporated with Mr. Bright's. was a system of laying water-pipes, just as gas-pipes are laid, nder the floors, which came at certain points through the ing of rooms, to which caps were held by a very soft solder pable of being melted at 120° or 130° of heat. The nozzle of se projections came just below the plaster of the ceiling, and n the heat in the room was sufficient to melt the soft joints cap fell off and holes were opened out from the pipes, through h water would spirt as from the rose of a watering-pot, and e pressure of the water it rotated, spreading the water. The

system was very strongly advocated at the time, and was said to have had very favourable reports from abroad.

Another point with regard to fire-alarms for large buildings is the difficulty experienced in getting a number of bells to ring on one wire. To obviate this I have adopted a system by which 100 or 500 bells on one wire can be rung at the same time from any point. No doubt all present know the action of a trembling bell, in which the back spring carries the current and the hammer in coming forward breaks it, and so on, to make a continuous action.

In this system a second spring is introduced on the opposite side of the hammer, and the two springs are so arranged that (the hammer) in its movements makes contact with each spring alternately for about the same duration of time. The connections are altered so that the hammer is always connected with the line wire, and when it is moved forward it comes in contact with the additional spring which is connected with the earth, and we thus get an intermittent discharge of the line wire to earth every time each bell rings, and at every bell, so that any number of bells by this system can be put on a wire, each helping to discharge it.

Professor W. E. AYRTON: The only point that strikes me in connection with the extremely ingenious invention of Mr. Bright is the temptation put in the way of any who, seeing the glass, might become possessed of the inclination to break it and draw the handle. The rapid growth in the number of the alarm-posts will, of course, increase that temptation, and if any statistics exist, I would ask in how many cases people have given way to it.

Sir CHARLES BRIGHT: Several have tried the experiment of giving false alarms, but a sentence of imprisonment or a salutary fine has checked the mischievous folly. Mr. Spagnoletti has reminded us of a patented invention for an automatic system of water dispersion which has not yet been put into practice, and I do not think likely to be, on account of the excessive expense and the uncontrolled damage it might do; say, in the case of torrents of water being let loose accidentally upon the goods of Messrs. Howell & James, or Swan & Edgar.

Many fire-alarms and burglar-alarms have been patented, but I consider the system before the meeting is the simplest existing.

The water-spouting idea just referred to came from America, and I have in recollection a burglar-alarm which I believe hailed from the same quarter, and it was a system which I think could hardly be surpassed, for it provided that, by an electrical arrangement, when the burglar stood in position to open the safe, a trap-door under his feet should open and precipitate him into a cell below, where he would be safe till morning, when an indicator would show that the trap-door had been in action. The only defect in the arrangement seemed to be the absence of an automatic handcuffing arrangement when the burglar was trapped.

I am glad to find that, after a great deal of trouble, electrical fire-alarms are being taken up in the Metropolis. It is almost a sad thing to think how long a time people have been at work in order to get such an essential appliance accepted, but it is almost sickening to try to get any corporation, or vested interests of any kind, to do anything whatever.

Mr. R. Von F. Treuenfeld: At the very beginning of his paper, Mr. Bright referred to a paper which I had the pleasure to read before this Society on the same subject, in 1877. At that time I may say that fire-telegraphs were hardly known in Great Britain, and I laid before the Society a number of statistical data which clearly showed the importance of such a telegraphic institution. I am pleased to hear, although the work has been slow, that nevertheless there is a hope now, seeing that the matter has been taken in hand in the proper quarters, that a system of fire-telegraphs will sooner or later be established in at least the large towns of Great Britain.

If I may be allowed, I will briefly repeat the principal data which I laid before the Society in the paper referred to. I think the combination of these data, with that which has been given by Mr. Bright this evening, will impress upon the authorities and the public the importance of an efficient system of electric fire-alarms. My statistics showed that towns without fire-telegraphs had a percentage of serious fires amounting to 29; when the fire-engine stations were simply connected up by telegraph wires, that

percentage was reduced to 17; and when fire annunciators were fixed in a similar way to those described to-night, the percentage of serious fires became reduced to 4. Those are average figures from a very large number of official statistical reports from towns all over Europe and America. Another important item I then laid before the Society was the proportion between the population and the points of call from whence a signal could be sent by any one to the Fire Brigade. In Continental and American towns this proportion was from 2,000 to 3,000 of population to one call-point, while in England it is as high as 15,000 to 80,000 population to one call-point.

Now, bearing in mind these data, and taking them in combination with Mr. Bright's paper, the question has suggested itself to me whether it would not be within the sphere of our Council to form a Committee on Fire-telegraphs who should draw out a report to prove the efficiency of electric fire-alarms, and protection thereby of life and property, and to take steps to bring the matter before the proper authorities. Fire-telegraphs are not like electric light companies or similar concerns, seeing that they are for the safety and protection rather than the convenience of the public; hence it is not a matter for any company, but for the authorities, whoever they may be, to take the matter in hand. I should be pleased if the Council saw fit to accept my suggestion as regards a Committee on Fire-telegraphs, which could perhaps enlighten municipalities in the matter, of course abstaining from recommending any specific system, merely showing the real advantages connected with fire telegraphs generally.

The first fire-telegraphs were fixed in Berlin in 1849, and have worked constantly since that time.

I would say, in reference to Mr. Bright's paper, that on the Continent it is considered rather preferable to concentrate the command over the whole system to headquarters. From the plan before us this necessity does not seem so predominant. The command during a fire, as long as it is a small one, may of course be handled from district headquarters; but the Continental systems always prefer to have central headquarters from whence the chief command is issued.

Again, it is considered advisable and necessary to have the possibility of communicating by means of a telegraph key from the annunciator-box towards the engine-station. Mr. Bright does not mention this, and I am not aware whether the apparatus he has described will allow of such a communication. If it does not, I think it would be preferable that it should, because then messages could be sent to (not received from) the headquarters from the box, to say whether more engines, more men, etc., were wanted.

I have not had experience with Mr. Bright's system, but the idea rather suggests itself whether a system based on balance of resistances is not complicating the older and simpler systems which have proved so efficient during 35 years. Of course this would be a question which only a longer experience with Mr. Bright's system could prove, and especially if the circuits, which now seem to be only very small ones, became much enlarged, and a greater number of annunciators were introduced.

Mr. G. C. SPRATT : I would like to ask whether an error is not likely to take place from the call-post not being self-setting?—whether, if in case the handle was pulled out and pushed in again by an excited person rushing up to the post, the call would not be missed at the receiving station, simply because the receiver would be finding out on the rheostat at the time the circuit was restored by the second movement at the call-post? I agree with Mr. Treuenfeld, and think it quite right that there should be speaking communication from the call-post to the nearest Fire Brigade station, or to the central station, so that if the Fire Brigade had a call to a certain district, and that fire turned out to be a bad one, a message of any kind could be sent from the nearest call-post to the central or district station, whereas now it is necessary to send a man back to his district station, from whence he can telegraph to Captain Shaw at headquarters, which causes delay.

Mr. EDWARD BRIGHT, in reply, said : Mr. President,—Many important questions have been put, and I should like to refer to them. Mr. Spagnoletti gave us some interesting information with reference to the large number of fire-alarms actually in use

by the railway company with which he is so prominently connected. I omitted to mention instances in connection with places where the self-acting arrangement of the system had been established. At the North Shore Mills, Liverpool, where more than 200 self-acting heat-detectors, or thermostats (I am not quite sure which is the proper term—"stat" does not give an indication of *movement* to my mind—I think heat-detector is perhaps the better term) have been fixed throughout the mill-rooms for several years, and no less than three fires have been saved by them. With reference to the interesting application that Mr. Spagnoletti has mentioned as regards railway signalling-lamps, I may also point out one or two other directions where such applications may be useful. We all know the delicate manner in which some fruit is reared in hot-houses—peaches, grapes, pine-apples, and so on. Now, by a very simple application of this apparatus, a thermostat set, we will say, to 80°, which shall make contact at a variation of 5° on either side of that temperature, would warn the gardener (perhaps at some distance) in the middle of a cold night that the fires were going out and that his fruit was perishing. Another application is that of using such an apparatus with hydro-incubators, which have to be kept at a very careful and very close temperature; and if the eggs are to be hatched properly with such contrivances, there can be nothing better than the use of a thermostat, set carefully to the temperature requisite to produce chickens, so as to give warning of any variation.

In reference to Professor Ayrton's remarks, which have been partly answered by my brother, I may say that in Captain Shaw's report (for the year 1880) after these fire-alarms had been first introduced, he enjoined the public to consider that the alarms were for the common good, and referred to a number of false alarms having been given. Now, it is a very reasonable thing to suppose, and the fact is well known, that certain minds are always exercised by any novelty; and when the red posts were first put about the streets no doubt there were many people of a perverse disposition who felt inclined to experimentalise. But a few of them were punished by the magistrates, and the result was that

the following year Captain Shaw, in his report, referred to the fact that the public had really, to a certain extent, taken these fire-alarms under their own protection, apart from the police, and that a person who meddled with the system was very likely to be roughly treated by those about, and that there had been a great and important diminution in the false calls. In his last report, Captain Shaw does not refer to false calls on the fire-alarms, so I may assume that they have arrived at a minimum.

As regards Mr. Treuenfeld's remarks, I think he must not have quite heard what I said, for I distinctly read, referring to the number of districts, that the fire-alarm call-points were marked out on the map, that 140 call-points were connected with 21 fire-engine stations, and that the district stations were all connected by telegraph, so that additional aid, or any other communication, could be made as desired. I now show you [producing it] a map on which is marked the telegraphic communication—its appearance is like that of a spider's web—between the Fire Brigade stations. The extent of the speaking-telegraph system between the stations is so great, that if placed on the same map as that which shows the fire-alarm circuits, the latter would not have been very distinct, which would have been a result contrary to my intention to-night. Large gaps appear on the map between the various circuits: they are sometimes caused by the parks, such as Hyde Park; but I look forward to the day when those curious zigzag lines you see of the alarms will be very greatly added to.

In further reply to Mr. Treuenfeld, I think I cannot do better than give him a few of the lengths of the circuits, as I have the distances with me. London and the suburbs occupy a considerable area. If we take, for instance, the Holloway circuit, that is about 7½ miles long, the Hammersmith circuit is 7 miles long, and so on: the zigzags shown on the map vary from 3 miles to 8 miles each. There is no difficulty whatever in adding to the length of a circuit by this system, and I think that will be understood when I mention that 5 per cent. margin between the calls is quite sufficient.

As regards the question of calling by an excited person, it is possible that an excited person may give a call; but I think I may

say that 49 out of 50 of the calls are given by the police and by the fire-escape men, who understand perfectly what to do with the apparatus; and if a person is very much excited he would in the attempt very often not send a call at all. A person may see smoke and become excited, or his brain may become excited from some other cause, but still, if cases of such irregular calling do arise, there is no difficulty whatever in dealing with them, because when the handle is pulled out it is very easy to prevent it going in again until the Fire Brigade people come to it, by exactly the same collar arrangement that I originally suggested to the Metropolitan Board of Words. The glass door could be dispensed with, and there need be nothing in the post but the coil: a key with a collar could be used, which, when inserted, would be held by a spring to prevent its withdrawal, and, the key being numbered, the holder could be traced. There need be no limit to the number of keys distributed for each post.

I think I have answered all the points raised.

Mr. R. Von F. Treuenfeld: In saying that the efficiency of the balance system would have to be proved after enlarging the circuits, I certainly do not mean the length of ground occupied, but the number of annunciators in one circuit; and when I say that Continental fire systems prefer to concentrate the chief command of the brigade to the central station, I mean that every fire which takes place is reported to the central station, and the general disposition is commanded from there.

Mr. Edward Bright: The fact is, the system as arranged is that which has commended itself to the chief of our Metropolitan Fire Brigade. I would willingly see any number of calls. There is a model on the table of the first apparatus, which I showed some five years ago, on this system, in the form of a desk, which has 25 points; and there is no reason why the number of points should not be increased almost indefinitely.

The President: We have heard from Mr. E. Bright this evening a very clear and interesting account of his system of giving fire-alarms, and the full importance of the system which he has perfected has been fully brought out by him, and also by the points raised in the discussion. I need not enlarge upon the

points which have been discussed, but will simply ask you to give your thanks to the author for his interesting and important communication.

A hearty vote of thanks was accorded Mr. E. Bright for his paper.

The PRESIDENT: The next meeting will take place on the 14th February, when we hope to have a very interesting paper on "Some New Instruments for Indicating Current and Electromotive Force," by Mr. R. E. Crompton and Mr. Gisbert Kapp.

A ballot took place, at which the following were elected:—

As Foreign Members:

Ichisuke Fujioka, M.E. | Hatsune Nakano, M.E.
Kosaku Kumakura, M.E. | Professor A. Stoletow.
Masahide Yoshida.

As Members:

Henry Clifford. | John Muirhead.
Professor A. W. Reinold, F.R.S.

As Associates:

Harry Theodore Barnett. | William Alfred Holmes.

As Students:

George Edwin Fletcher. | George Jules Manington.

The meeting then adjourned.

The One Hundred and Thirtieth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 14th, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates were announced and suspended.

Donations to the Library of the Society were announced as having been received from Imperial Russian Telegraph Department; Charles Todd, Adelaide Observatory; C. L. Madsen; Société Internationale des Électriciens, Paris; the Postmaster-General; Philosophical Society of Washington; and a vote of thanks was awarded to the donors.

The PRESIDENT: I regret to have to announce to the meeting the death of Mr. Frank Ives Scudamore, one of our Honorary Members, and the second President of our Society.

The following paper was then read:—

ON SOME NEW INSTRUMENTS FOR INDICATING CURRENT AND ELECTRO-MOTIVE FORCE.

By R. E. CROMPTON and G. KAPP.

In consequence of the rapid development of that part of electrical science which may be termed "heavy electrical engineering," reliable measuring instruments, specially suitable for the large currents employed in lighting and transmission of energy, have become an absolute necessity.

As usual, demand has stimulated supply; and many ingenious and useful instruments have been invented, the manufacture of which forms at the present day an important industry.

Mr. Shoolbred, in a paper which he recently read before this Society, gave a full and interesting account of the labours of our predecessors in this field.

To-day we add to the list then given a class of instruments invented by us, examples of which are now before you on the table. We have preferred to call them current and potential

indicators, in preference to meters, considering that the latter term, or rather termination, ought to be applied rather to integrating instruments, which the necessities of electric lighting are likely to soon bring into extensive use.

The principal aim in the design of these indicators has been to obtain instruments which will not alter their calibration in consequence of external disturbing forces. If this object can be attained, then it will be possible to divide the scale of each instrument directly into ampères or volts, as the case may be, and thus avoid the use of a coefficient of calibration by which the deflection has to be multiplied. This is an important consideration, when it is remembered that in many cases these instruments have to be used by unskilled workmen to whom an arithmetical calculation involving the use of decimal fractions is a tedious, and in some cases even an impossible task.

All measurements are comparative. We measure weights or forces by comparison with some generally known and accepted unit standard weight; lengths, areas, and volumes by comparison with a unit length; resistance by a standard ohm, and so forth. In the same way, currents could be measured by comparison with a standard current, but this would be a troublesome process, not only on account of the apparatus necessary, but also because it would be a matter of some difficulty to have a standard current always ready for use. In general, measurement of current and potential by direct comparison with a standard unit is discarded for the more indirect method of measuring, not the current itself, but its chemical, mechanical, or magnetic effect.

The chemical method is very accurate if a proper density of current through the surface of the electrodes be used,* but since it requires a considerable time, and, above all, an absolutely constant current, its use is almost entirely restricted to laboratory work, and to the calibration of other instruments. For practical ready use instruments employing the mechanical or magnetic effect of the current are alone suitable. We weigh, so to speak,

* According to recent experiments made by Dr. Hammerl, the density of current in a copper voltameter should be half an ampère per square inch of surface.

the current against the force of a magnet, of a spring, or of gravity.

The measurement will be exact if the thing against which we weigh or counterbalance the current itself retains its original standard value. Where permanent magnets or springs are used as a balancing force, this condition of constancy in our weights and measures is not always fully maintained; and, to make matters worse, there is no visible sign by which a change, should it have occurred, can be readily detected. A spring may have been overstrained, or a steel magnet may have become weakened without showing the least alteration in outward appearance. To overcome this difficulty, the obvious remedy is not to use springs or steel magnets, but to substitute for these some other force, which either should be absolutely constant,—such as the force of gravity,—or at least should vary only within narrow limits; and this variation should be in accordance with a definite law. This latter condition can be fulfilled by the employment of electro-magnets.

To imitate with an electro-magnet a permanent magnet as nearly as possible, so that the former can be used to replace the latter, it is necessary that the magnetism in the iron core should remain constant. This could of course be done by exciting the electro-magnet with a constant current from a separate source. (In a recent note to the Paris Academy of Science, M. E. Ducretet described a galvanometer with a steel magnet which is surrounded by an exciting coil. When recalibration appears necessary, a known standard current from large Daniell cells is sent through this coil during a certain time, and thus the magnet is brought back to its original degree of saturation. M. Ducretet also mentions the use of a soft iron bar instead of a steel magnet, in which case the current from the Daniell cells must be kept on during the time an observation is taken.) But such a system would appear to be too complicated for ready use; moreover, some sort of indicator would be required by which we could make sure that the exciting current has the normal strength.

The plan we adopt is to excite the electro-magnet by the whole or a part of the current which is to be measured. Since

this current varies, the power exciting the core of the electro-magnet must also vary; and since we require the core to have as nearly as possible a permanent magnetic force, we are brought face to face with the question whether an electro-magnet can be constructed that has a constant moment under varying exciting currents.

This question has been answered by the well-known experiments of Jacobi, Dub, Mueller, Weber, and others. To get an absolutely constant magnetic moment is not possible, but between certain limits we can get a very near approximation to constancy. The relation between exciting power and magnetic moment is very complicated, depending not only on the dimensions and shape of the core and the manner of winding, but also on the chemical and molecular constitution of the iron of the core. It is not possible, or at least it has hitherto not been found possible, to embody all these various elements into an exact mathematical formula which would give the magnetic moment as a function of the exciting current; but the above-mentioned experiments have shown that within certain limits, and in the neighbourhood of the point of saturation, the relation between the two is that of an arc to its geometrical tangent (Fig. 1). It will be seen that for large angles the arc increases much slower than the tangent—that is, for strongly excited cores a very large increase of the exciting current will produce only a slight increase of magnetic moment.

If Mueller's formula were correct for all currents, absolute saturation could only be reached with an infinite current. Whether this be the case or not, it is certain that the greater the exciting current the less will a variation in it affect the magnetic moment of the core.

To imitate as nearly as possible permanent steel magnets, it is therefore necessary to use electro-magnets the cores of which are easily saturated. The core should be thin and long, and of the horse-shoe type: the amount of wire wound round it should be large in comparison with the size of the core.

Here is a magnet partly wound, which was used in one of our earliest experiments, but which was a failure on account of having far too much mass in the core in comparison with the amount of

copper wire wound round it. Since then we have greatly diminished the iron and increased the copper. The cores of the instruments on the table are composed of two or three No. 18 B.W.G. charcoal iron wires, and are wound with one layer of ·120-in. wire in the case of the current indicators, and eighteen layers of ·0139-in. wire in the case of the potential indicator.

If from the diagram, Fig. 1, we plot a curve the abscissæ of which represent exciting current, and the ordinates magnetic moment of the soft iron core (Fig. 2), we find that a considerable portion of the curve is almost a straight and only slightly inclined line. If it were a horizontal straight line the core would be absolutely saturated; but such as it is it answers the purpose sufficiently well, for, with a variation of exciting current from 10 to 100 ampères, the magnetic moment varies but slightly. If a small soft iron or magnetic steel needle, *n s* (Fig. 3), be suspended between the poles, *s n*, of an electro-magnet of such proportions as described above, and the current after exciting the electro-magnet, *e e*, be led round the coils, *d d*, it will be found that for all currents between 10 and 100 ampères the needle, *n s*, shows a definite deflection for each current.

Here we have a galvanometer with permanent calibration. In this case the deflection of the needle will not strictly follow the law of tangents, because the controlling power of the electro-magnet is not absolutely constant; but whatever the exact ratio between deflection and current may be, it must always remain the same, and to each angle of deflection corresponds one definite strength of current. The force with which the electro-magnet tends to control or keep the needle in its zero position—that is, in line with the poles, *s n*—is due partly to the magnetism of the core, which is nearly constant, and partly to the magnetic influence of the coils, *e e*, themselves, which is of course simply proportional to the current.

The total magnetic force acting on the needle is therefore represented by the sum of these two forces, and consequently not nearly so constant as might be desired in order to get a good imitation of a tangent galvanometer with a permanent magnet. In the diagram, Fig. 2, the curve, O A B, represents the magnetic

moment of the iron core, the straight line, O D E, that of the exciting coils *per se*, and the dotted line, O F M, the sum of the two obtained by adding for every current, O C, the respective ordinates C D and C A.

$$C F = C D + C A.$$

The rise of this curve shows that the force which tends to bring the needle back to its zero position increases with the current, though at a slower ratio than the deflecting force of the currents. It follows from this, that for large currents the increment in the angle of deflection is comparatively small, and the divisions on the scale whereon the current is to be read off would come too near together to allow accurate readings to be taken. In other words, the range of accurate reading in an instrument so constructed would only be limited. But it is very easy to eliminate the magnetic effect of the coils of the electro-magnet on the needle by introducing an opposite magnetic effect, so that only that part of the force remains which belongs to the soft iron core proper.

One way of doing this is by surrounding the needle with a coil the plane of which is at right angles to the line *s n*, and coupling this coil in series with the deflecting coil, *d d*. If the proportions of this transverse coil and the direction of the current through it be properly chosen, its magnetic effect can be made to exactly counterbalance that of the exciting coils, *e e*, without perceptibly weakening the magnetism of the iron core. But instead of employing two coils, one parallel and the other transversely to the zero position of the needle, we can obtain the same result in a more simple manner with one coil only, if this be placed at such an angle that its magnetic effect can be substituted for the combined effects of the two coils. In other words, we set the deflecting coil, *d d*, at a certain angle to the zero position of needle.

A similar arrangement, though not precisely for the same purpose, has already been suggested and tried by Messrs. Deprez, Carpentier, Ayrton, and Perry, in galvanometers with permanent steel magnets.

If the coil, *d d*, be so placed, the deflecting force, which now acts obliquely, can be considered as the resultant of two forces—

one acting at right angles to the line $s\,n$, as in an ordinary galvanometer, and the other parallel to this line, but in a sense opposed to the action of the electro-magnet and its exciting coils. If the angle of obliquity be so chosen that this latter component exactly equals the magnetic effect of the exciting coils *per se*, an equality which holds good for all currents, then we shall have an almost perfect imitation of a tangent galvanometer with permanent magnets. But we can go a step further than this: we can overbalance the exciting coils by setting the deflecting coil at a greater angle than necessary for the mere elimination of the former, and thus attain that an increase of current results in a slight weakening of the field in which the needle swings, thus allowing the increment of the angle of deflection to be comparatively large even for large currents. In this way it is possible to obtain a more evenly divided scale than is the case when the deflection follows the law of tangents, as in an ordinary tangent galvanometer.

This principle of overbalancing the exciting coils is shown in diagram (Fig. 2.)

The straight line O G represents the magnetic effect on the needle of that component of the deflecting force which is parallel, but in sense opposed to S N, as mentioned above; the magnetic effect of the exciting coils is represented by the straight line O E. The combined effect of these two forces on the needle is represented by the line O K, the ordinates of which must be deducted from those of the curve O A B, in order to obtain the total directing force due to each current.

This is shown by the curve O P Q, shown in a thick, full line. This curve shows how the directing force or strength of field in which the needle swings decreases with an increasing current.

That this does actually take place can easily be proved by experiment. Fig. 4 shows two curves: the one drawn in a full line is obtained by plotting the deflection in degrees of the needle of a potential indicator as abscissæ, and the corresponding electromotive forces measured simultaneously on a standard instrument as ordinates; the dotted line shows what this curve would be with an ordinary tangent galvanometer.

The needle of the potential indicator is mounted at the lower

end of a steel axle, to the upper end of which is fastened a light aluminium pointer, whereby the deflection of the needle can be read off on a scale divided directly into volts.

The scale is placed within a circular dial-plate, with glass cover, giving sufficient room for the pointer to swing all round, and the needle is placed within a central tube, fitting it closely, which acts as a damper, and so makes the instrument almost dead-beat. Tube and dial are in one casting. The electro-magnet is of horse-shoe form, fastened to a central tubular stand, which also serves to support the two deflecting coils, one on either side of it. The tube within which the magnetic needle swings is inserted into the stand, which is bored out to the external diameter of the tube. The electro-magnet and deflecting coils are wound with from 50 to 100 ohms of fine insulated copper wire, and an additional resistance coil of from 450 to 900 ohms of German silver is added, which can, however, be short-circuited by depressing a key when the instrument has to be used for reading low electro-motive forces. In this case the indications of the pointer must be divided by ten. If a current be sent through the instrument the wrong way, the needle turns through an angle of 180°, and thus brings the pointer to the side of the dial opposite to where the scale is. In this position no reading can be taken; and, to facilitate the sending of the current in the right direction, a commutator is added, and is so coupled up that, when the pointer stands over the scale, the handle on the commutator points to the positive terminal screw.

Since the core of the electro-magnet requires a certain minimum of current to become saturated, there is a limit of electro-motive force below which the indicator fails to give reliable readings. For instance, an instrument wound with 100 ohms of copper wire and 900 ohms of German silver, can be used for electro-motive forces varying between 300 and 3 volts, but would not be reliable for measuring less than 3 volts.

For very exact measurements the instrument should be placed north and south, in the same position in which it was calibrated, and it should not be placed close to wires carrying strong currents, magnets, or masses of iron.

Two different patterns of current indicators are on the table—one with double needles suspended on a point in the way compass magnets are suspended; the other with one lozenge-shaped needle mounted on an axle and pivoted in jewels, in every way similar to the needle of the potential indicator first described.

For measurements of currents from ten ampères upwards, there is no need to employ a complete coil as the deflecting agent: one half coil or one strip, passing close under the needle, gives sufficient deflecting force, and thus the construction of the instrument is rendered extremely simple.

The current, after entering at one of the flat electrodes, splits in two parts, each part passing round the winding of an electro-magnet of horse-shoe form, the similar poles of both magnets pointing towards each other and towards the needle.

After traversing the winding, the current unites again and passes through a metal strip close under the needle, and finally out of the instrument by the other electrode, which lies close under that at which the current entered, but is insulated from it by a sheet of fibre.

The metal strip is set at an angle to balance or overbalance, as may be preferred, the magnetic influence of the exciting coils.

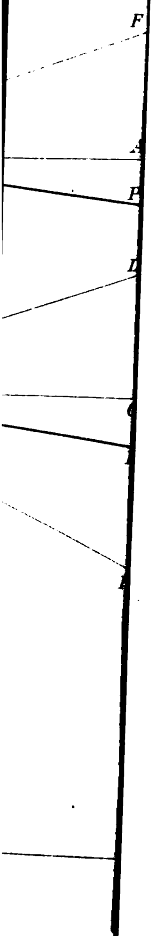
The effect of this overbalancing is shown in Fig. 5, where the full curve represents the current as a function of the deflection obtained by comparison with a standard instrument, and the dotted curve shows what that relation between deflection and current would be if the law of tangents held good for these instruments.

It will be seen that about the middle of the scale the dotted line coincides nearly with the full line, whilst at the extreme end of the scale the dotted line is higher.

The result of this being, that if we compare our indicator, from which this curve was taken, with any form of tangent instrument showing an equal angle of deflection at the medium reading, it will be seen that the needle of our indicator will be deflected to a greater angle at high readings than that of the tangent instrument consequently the divisions on the scale will be wider apart in our instrument, which greatly facilitates high readings.

TO MESS[RS]

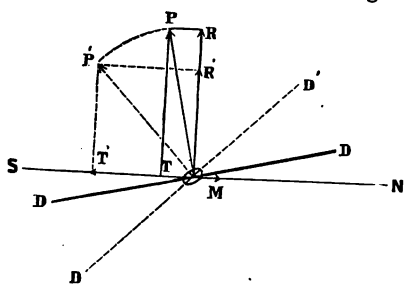
Fig 2



The PRESIDENT: We have just had a most interesting paper read by Mr. R. E. Crompton, and as we have plenty of time before us this evening to discuss this interesting subject, I hope we shall have a very good discussion upon it. Instruments for the measurement of current and electro-motive force are very much required at the present time, and Mr. Crompton has very clearly explained the principle of these which he has introduced to us. Before the discussion commences, I will ask Mr. Gisbert Kapp if he would like to add any remarks on the subject of the paper.

Mr. GISBERT KAPP: I might perhaps explain, by the aid of the parallelogram of forces, a little more clearly what is meant in the paper by the action of "overbalancing."

The diagram before you is perfectly right as far as it goes, but it will be more clearly illustrated by the following diagram:—



Let S N be a line drawn through the poles of the electro-magnet, and let D D represent the plane of the deflecting coil.

The magnetic effect exerted by the current in D D upon one pole of the needle is a force O P, which can be considered the resultant of the two components O R and O T. The latter is in sense opposed to the force O M, which represents the magnetic effect of the coils of the electro-magnet alone upon that pole of the needle.

Now, if O T = O M, we have a perfect balance at all currents. Imagine that by employing sliding contacts it were possible to turn the deflecting coil into the position D′ D′ without altering the current.

The magnetic effect would then be O P′, in magnitude equal

to O P, but acting at a different angle, and its components O R′ and O T′ would be different to those corresponding to the first position; O R′ being smaller and O T′ larger. The difference between O T′ and O M shows by what amount the magnetic effect of the electro-magnet has become weakened, and in this condition we call the instrument overbalanced.

I need hardly point out that in our instruments, as in any tangent galvanometer, the strength of magnetism in the needle has no influence upon the angle at which it will set itself when a current is passing through the instrument.

A non-magnetised soft iron needle could be used, and in fact has to be used for measuring alternating currents, but then the power to overcome friction is not so great, and therefore the movement more sluggish. With a magnetised steel needle the instrument has a more decided action, and the needle jumps more readily into position.

Dr. J. H. HOPKINSON: I am afraid I am not able to say anything very valuable upon the paper. In dealing with questions of this kind, it is necessary that one should have experience of the working of the instrument before us, which in this case I have not had. I will, however, take the opportunity of relating my experience with Sir William Thomson's instruments, two or three of which I have had in constant use for more than twelve months, both for potential and current. The magnets have had their constancy tested pretty frequently, for the purpose of making quite sure that no serious change had occurred in them; and the result has been that with one of them the range of its constancy, in relation to the coil in which it is used, has been from 11·3 to 11·8 (not with one observation, but perhaps in thirty or forty trials); in the other the range was from 9·5 to 9·8. Those results show that the magnets in Sir William Thomson's instruments vary very little indeed. The objection to such instruments, and indeed to the majority of galvanometers used for the measurement of current, is the degree to which they are disturbed by strong magnetic forces in their neighbourhood. This difficulty has, I believe, been removed by Sir William Thomson, by altering the mode of suspension and using an

astatic needle. Siemens' electro-dynamometer is open to the same objection; and a great deal of care is required in using that instrument or Sir William Thomson's, in order to avoid erroneous results from the effect of magnetic fields, which may be very intense and have a material effect, although the distance from the instrument may be very considerable.

The PRESIDENT: At the Royal Society to-day we have had a paper bearing on the same subject from Professors Ayrton and Perry, and Professor Ayrton will probably wish to give us his ideas on this subject.

Professor W. E. AYRTON: Mr. Crompton has given us a most interesting paper, and one of the greatest practical importance. If I might be allowed to do so, I would differ a little from Mr. Crompton when he says the word "meter" ought to be applied only to an instrument which integrates. I do not see any necessity for so confining the word, especially when it is considered that the words thermometer, barometer, hygrometer, hydrometer, and so on, have nothing whatever to do with integration. If the proper expressions are used, viz., a coulomb-meter or an erg-meter, when instruments integrating the total quantity of electricity or work are spoken of, I see no reason whatever why ammeter or voltmeter should not be used when current or electromotive force is measured, nor why we should not regard the word "meter" as the termination of the names of "measuring" instrument. The name "indicator," which Mr. Crompton has suggested, would imply that the instrument only indicated more or less, and did not measure the amount; but I am sure that neither Mr. Crompton nor any one else would wish for a moment to apply a name to the very ingenious instruments he has been good enough this evening to bring to our notice which could possibly imply that they did not measure.

The question of making instruments which do not require the use of constant or any table of values is one undoubtedly of immense practical importance, for the reasons Mr. Crompton has given, viz.: very often people who are in charge of electric light circuits, not being remarkably well versed in mental calculation, are unable to multiply a constant of 2·73 by a deflection of 13·25

without pen and paper; and further, unless the dial of the instruments indicates in unmistakable figures the value of the thing measured, the user of an instrument is very liable to use the wrong constant, and the result of that is, that not only is a totally wrong impression derived of the value of the current or of the potential difference measured, but frequently either the instrument is destroyed, or at any rate the pointer broken, from an instrument suitable for being used to measure a small current or potential difference being by mistake used to measure a very large one. Some of the members present may perhaps, however, be aware that some three weeks ago, at the Physical Society, Professor Perry and myself made a communication on "Direct Reading Instruments" with which no constant nor table of values was necessary to be used, but on which the current or electro-motive force was read off just as the number of pounds pressure per square inch in a boiler is read off on a pressure gauge, without any reference to a table. Mr. Crompton has opened up a very important question in considering the permanency of springs, and whether they can be used for electric measuring instruments. He has suggested gravity, which has been employed as the controlling power for a measuring instrument. I am not sure that we can say straight off that springs cannot be made permanent. The accuracy of chronometers is based mainly on the permanency of a spring, and we know that chronometers can be timed so as to go for long periods with a very considerable degree of accuracy, and to have a well-known perfectly constant rate of gaining or losing; whereas, if there were any unknown even small error due to want of permanency, or due to irregularities in the balance-spring, this would develop to an enormous error in a month or two, which could not only not escape notice, but which would very probably lead to the loss of the ship. No such error is, however, found to arise in a good chronometer, which in fact is the most perfect measuring instrument of any kind that we possess, and therefore I venture to think that springs can be made which will have a very large amount of constancy. As a matter of fact, I remember some years ago that Sir William Thomson was making springs which were to be tested at some thousand years hence,

for the purpose of detecting the change in the earth's period of rotation, so as to measure the increase in the length of day arising from tidal retardation. Of course there must be a temperature correction for any spring, but that is a perfectly definite thing and can be easily allowed for.

Undoubtedly, if the spring, as in the Siemens' dynamometer, is exposed to air currents as well as to meddlesome fingers, and if, further, it supports a piece of wire not only of large weight, but so placed as to have a large moment of inertia, we cannot expect the indications of the spring to have any large amount of constancy; but if the spring, as in the case of a watch, or as in the case of our new measuring instruments, is well protected from outside disturbance and damage, then I expect its indications may be very permanent.

Dr. Hopkinson has referred to the permanency of magnets. Magnets have been made for some of our measuring instruments which were by no means permanent horse-shoe magnets; and I think that this arose a good deal from our having used armatures with our instruments, and from these instruments being constantly put on and withdrawn. We have been compelled to think about this subject a great deal during the last few months, and the conclusion we have arrived at is, that the use of an armature is distinctly bad for a measuring instrument, especially an instrument that is used every day, and therefore from which, if there be an armature, it is being constantly withdrawn with a certain sharp pull and put on again equally suddenly. If you are going to put an instrument on one side for many months, and that instrument contains a permanent magnet, then possibly the use of an armature may be beneficial; but if you are going to use it daily, it is, I fear, worse than useless. It has been, I believe, the careful putting on of the armature each time the current has been stopped passing through our instruments that has caused the permanent magnets in some of them to lose their power to a considerable amount in a comparatively short time; while with others of our instruments which have probably been used carelessly,—that is, no one has taken the trouble to put the armature on,—the permanent magnets have not lost one per cent.

in many months, as can be ascertained by the constant of the instruments having maintained an astonishing degree of permanency. Sir William Thomson's instruments are not provided with armatures; and one of my students recently told me that, thinking an armature might be a good thing, he used one, but found that its employment caused the strength of the permanent magnet to fall with great rapidity. He wrote to Sir William Thomson, who replied that on no account should an armature be used with a permanent magnet which you want to keep constant in its strength, but that it should be hung up so that its poles were in the line of dip. I think it possible, therefore, that even a permanent magnet may be made with a marvellous degree of constancy, but certainly I feel tolerably sure that springs can be so made.

The objection to the use of gravity, which for our rough practical purposes may be regarded as fairly constant, is the want of quickness of action, so that any small temporary change in the strength of the current being measured is not instantly recorded. For this purpose the needle and pointer must not only be very light, but the controlling force must be great. Now, if gravity be used, the only way to obtain a large controlling force is to use a large mass to be attracted, but if a large mass be attached to the needle and pointer, the moment of inertia will be seriously increased, and slow motion will be the result; whereas, by using a powerful controlling magnet or a comparatively strong spring, we obtain a dead-beatness so great that the number of times the joint in the driving-belt passes over the dynamo pulley can be easily counted, every adjustment in the carbons on an arc lamp seen on the ammeter and voltmeter, and even the effect on an arc lamp of whistling may be instantly observed on the distant ammeter.

Some time ago my colleague and myself were considering how to obtain some mode of controlling which was not likely to be altered, and we did not see our way, either by the use of gravity or by using the current itself, to produce a powerful controlling force so as to obtain a considerable rapidity of vibration; and to avoid the needle being very liable to be affected by outside influences, which happens if the controlling force is weak. Damping the vibrations can of course be produced by a vane moving in oil or in

the air, or by magnetic friction arising from currents being induced in neighbouring metal, but such a mode of damping the vibration does not afford any shielding from extraneous magnetic action. The idea of using a piece of iron and saturating it is an extremely ingenious suggestion on the part of Mr. Crompton. And this is the plan that we have employed in the instruments which the President has mentioned as having been brought before the Royal Society to-day. For the purpose of getting the proportional law in our instrument, there is a small piece of iron which is attracted into a solenoid, and by making the piece of iron extremely small a very small current instantly saturates it, and a deflection is obtained proportional to the current. The idea, therefore, of quick saturation had not escaped us. But I still do not quite see how, without a powerful controlling force and without any iron screen, Mr. Crompton can prevent his instrument being seriously affected by neighbouring dynamos. I have no doubt that his ingenuity will enable him to get over the difficulty; but it seems to me that with his deflecting coils so far away from the needle, as they are in the instruments before us, his deflecting field must be very weak, hence his controlling field must be equally weak, and I fear that a few feet above a dynamo machine you would be liable to get considerable errors arising from the magnetic disturbance set up by the dynamo in action. In fact, we have found it necessary, not merely to use instruments of which the swing of the needle is very rapid, but to go further, and surround the instrument with an iron shield, so as to avoid the interference which arises from the very powerful magnetic fields present in a dynamo room.

Dr. Hopkinson has referred to the Siemens dynamometer, and the difficulty arising with it from disturbance. A very curious example of that difficulty occurred some years ago when we were first testing some of our ammeters. We calibrated it with some standard Daniell cells and then compared with a Siemens' dynamometer, but were unable to get the same result from the dynamometer as from the cells. I ascertained from Messrs. Siemens that their dynamometer instrument had been calibrated more or less carefully by the deposition of copper, so that the discrepancy

seemed without explanation when it was found out that the cause arose from the powerful controlling permanent magnet of our own instrument affecting the dynamometer. Consequently, when the Siemens dynamometer was turned round so that the lines of force crossed it in the opposite direction, the error was reversed, and we got a discrepancy in the opposite direction.

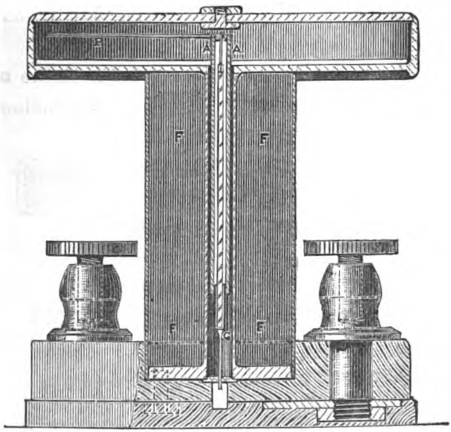
Your President has asked me to describe our two new measuring instruments, exhibited this afternoon at the Royal Society, which I by chance have with me on my way home from the Royal Society. The peculiarity of the instrument is that we are able to have an extremely large deflection, not merely a deflection of 35° or 40°, which is the common thing with an ordinary galvanometer, but a deflection of 270°, strictly proportionate to the current, excepting for the first few divisions, during which portions of the scale the current is saturating the small soft iron core.

The special novel feature in these instruments is the dispensing with a permanent magnet, which may not be so permanent as desired, and the employment of a new form of spiral spring that we have devised, of such a nature that for a small axial extension there is a large amount of rotation of the movable end of the spring relatively to the fixed one. In an ordinary spiral spring there is of of course no such rotation. The result is that without the employment of a rack and pinion, or of levers, or of any other magnifying arrangement, and without, therefore, the cost or the friction attending the use of such magnifying arrangements, we are able to obtain extreme delicacy.

One form of our new type of ammeter is shown in the following figure, where A A is a thin hollow tube of charcoal iron, attached at its lower end to a hard piece, G, guided at the bottom in the way shown. To G is attached the lower end of a spring made like a very long thin coiled-up shaving of silver or hard phosphor-bronze, the upper end of which is attached rigidly to the glass top of the instrument, which itself is fastened rigidly to the framework of the instrument. This piece attached to the glass, and to which the upper end of the spring is attached, also serves as a guide to the top of the iron tube. In the space F F a solenoid wire or strip is wound, its ends being attached to the terminals shown.

Hence, when a current is passed through this solenoid, the iron tube is sucked into the solenoid, and its lower end, G, to which the spring is attached, receives a large rotatory motion, which is communicated directly to the pointer attached to the top of the iron tube. Parallax, in taking readings of the pointer, is avoided by the horizontal scale being on looking-glass in the well-known way.

By making the iron tube A A very thin, so that it is magnetically saturated for a comparatively weak current, by fixing it so that it projects into the solenoid, a definite distance which has been carefully determined, partly by calculation and partly by experiment, and by constructing the spring in conformity with the conditions which we have worked out, and which are given fully



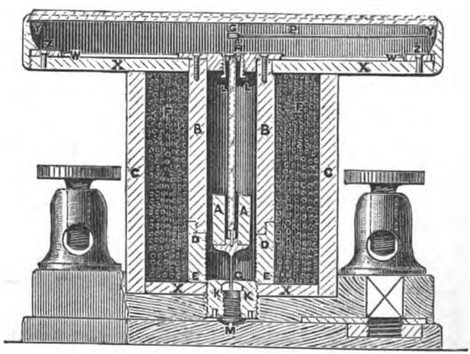
in the paper read before the Royal Society this afternoon, for obtaining a large rotation with the minimum stress, and with not too much axial motion of the free end of the spring, we have succeeded in obtaining deflections up to 270°, which, with the exception of the first 7°, are directly proportional to the current, and without any permanent set being given to the spring.

And it may be mentioned that, in order to avoid a spring taking a permanent set for a large deflection, it is of great importance that the spring, after being delivered by the maker, should receive a large degree of permanent set in the direction in which we wish it to be afterwards strained in ordinary working.

This instrument is "direct reading," and its sensibility is initially adjusted, partly by the amount of wire or strip wound on the bobbin, and partly by means of a small movable coil, the proper position for which is determined in each case experimentally by the makers, and there it is permanently fixed.

This form of ammeter is far better shielded from extraneous magnetic influences than any of our earlier types, which latter are also much better screened from magnetic disturbances than those of Sir William Thomson, where the coils are large and far from the needle. Still we have found it desirable to go yet further in this screening; and we have succeeded in constructing an instrument that can be used without practical error, not merely near a dynamo or motor when working, but actually on the top of the dynamo itself.

The special form of construction is shown in the next figure, and consists in making the electro-magnet of a hollow core, part



of which, B B, is of charcoal iron, and part, D E, of brass or other non-magnetic metal. The outside tube, C C, and the plates, X X, top and bottom, are also of charcoal iron. The space F F is filled with insulated wire in electrical connection with the terminal, so that when a current is sent through the instrument an intense magnetic field is formed between it and E, which are the poles of the electro-magnet. To the iron tube A A, also made of charcoal iron, the spiral spring, in this case made of extremely thin hard

steel, is attached, the other end being attached to the piece F, which is fixed relatively to the bobbin. The spindle G G, which is fixed to the moving iron core A A, moves freely in bearings H H, so that the only movements of which A is capable are one of rotation and one parallel to the axis of the bobbin. As the iron core A projects into the strong magnetic field between D and E, it is strongly attracted towards E when the current flows, and, as before, causes a large rotation of the pointer, P, over the scale. As a means of varying the power of the instrument, an adjustable iron piece, K, is provided, which can be screwed nearer to, or farther from, the core A, and by the use of which the sensibility of the instrument can be adjusted so as to make the instrument also "direct reading," that is to say, each division of the scale can be made to correspond with 1 ampère of current, or 1 volt difference of potential.

These magnifying spring instruments in their earliest forms only indicated the magnitude of the current or potential difference in ampères or volts respectively, and not the direction of the current. In our latest instruments, however, we have added a small compass needle, which indicates the direction of the current, while the principal pointer, as before, indicates the magnitude of the thing to be measured.

Professor SILVANUS P. THOMPSON: I have made some experiments with springs for the purpose of constructing a measuring instrument, but I have not by any means travelled over the same ground as Professors Ayrton and Perry have done, and I have not arrived, as they have arrived, at a very fine and practical form of instrument: my experiments, which related to an integrating electric meter, have been left as yet in the experimental stage. But in the course of my experiments I was led to enquire into the question of constancy, and, not being an engineer with the experience of half a century or more at my back, to tell me all that could be told about springs, I had to enquire. I put the question to the late Mr. W. Froude, within a few weeks before his death, whether he could give me any information on the question of the constancy of springs. He told me that he had got steel springs in use which he had tested

at periods over 20 years, and that he could entirely rely upon their efficiency as long as they were not overstrained. That was his opinion of steel springs; and I am inclined to think that German silver springs would prove themselves to be equally reliable if they are properly treated. I have myself used phosphor-bronze springs for measuring small magnetic forces. In connection with the question of the permanency of springs, I would like to allude to the question of the constancy of permanent steel magnets. I have met over and over again, in using Messrs. Ayrton and Perry's measuring instruments, with the very same difficulty, that the magnets did not retain their constancy. I attributed it to the same reason, that the continual careless pulling off and putting on of the armature affected the constancy. But if Professor Ayrton will allow me the criticism, the weakening effect is not due to the pulling off of the magnet, but to the putting of it on. I have made some experiments with a large horse-shoe steel magnet, to see what the effect was of pulling off and putting on the armature. I came to the conclusion that it was a great deal worse to put on the armature suddenly than to pull it off suddenly. If you pull off the armature of a magnet as quickly as you can, then after you have pulled it off put it on gently at the bend of the magnet and slide it along to the polar ends, and then pull it off suddenly, and so on a good many times in succession, you will find, instead of diminishing the strength, it is increased; whereas, on the other hand, if you put on the armature suddenly, by allowing it to be attracted with a knock against the poles, and slide it off gently at the bend, that diminishes the magnetism. That is indeed what we should expect from the law of conservation of energy, and from Lenz's law of induced currents. The induced currents set up in the steel magnet itself will always be such as to oppose the movement that is inducing them. Therefore, if we put on the armature very suddenly, we shall have a demagnetising current, while if we pull off the armature we shall have a current which tends to strengthen the magnet and make it pull more strongly. I think that is an explanation of a great part of the effect of decrease or increase in the strength of the

magnets when they are so abused. Whether this is the whole cause I do not know. Some light will probably be thrown on the matter by Professor Hughes' recent work on the changes that take place in the inside of a permanent steel magnet when subjected to various external forces.

Now let me say a word to connect together the observations I have made about springs and magnets. There has been published very lately, by Professors Strouhal and Barus, in the pages of *Wiedemann's Annalen*, a number of researches upon the constancy of magnets. They have tried to find out what process of magnetisation will give the most constant and the most powerful result, and their conclusion is—I speak from memory only—that the steel should be tempered at a bright red or nearly white heat in the usual way; it should then be heated in steam for some hours, then cooled, then heated again to the same temperature for several hours, and that process repeated several times. By this means a magnet is obtained whose permanent magnetism is rendered by this process of quasi-annealing in steam much more constant than without any such process. I do not know whether engineers can tell us of anything analogous to that in the behaviour of steel springs subjected to a recurrent heating in boiling water, and being thus annealed.

The advantage of direct-reading instruments over those which require continual calculation and frequent recalibration is enormous, as Mr. Crompton and Professor Ayrton have remarked. I was, I believe, one of the first in England to use the very beautiful instruments which Sir William Thomson has devised for measuring potential and current, and I found there was much unnecessary trouble in having to perform the calculations, which in this case were to multiply the scale-reading of the needle by the value of H, and divide by the platform-reading. I soon gave up the platform-reading, and calculated one for myself which would obviate future calculations. I first put on a standard battery that would give me a known potential. I adjusted the magnetometer part of the instrument to such a position on the platform as made the scale-reading the exact figure in volts, and marked for myself a new platform reading as "unity:" whenever

afterwards I adjusted the instrument to the same mark, I could read volts direct without calculation. That plan has been adopted, I understand, in the later instruments which have been made by White of Glasgow, for Sir William Thomson.

My only other remark is in reference to the instrument referred to by Professor Ayrton as being enclosed in an iron shield. I was once using one of Professors Ayrton and Perry's voltmeters in the middle of a lot of dynamos, and was much bothered by the influences of so many lines of force running all over the place. The controlling magnet ceased to control properly, and, having no other means at hand, I took a stout iron pot and put the voltmeter in the inside of it. I do not think that that would improve the calibration, but I took good care that the magnet did not touch the interior of the iron pot, and I found that I could cut off a good deal, though not the whole of the disturbances, in that way. I do not doubt that the new "voltmeter in a pot," if I may so christen the new instrument which Professor Ayrton has shown us to-night, will be a very useful instrument indeed under similar circumstances.

Professor AYRTON: The reason why our voltmeters are more liable to be affected by extraneous magnetic disturbances than our ammeters, is because we are compelled to be contented with a weaker controlling magnet for a voltmeter than for an ammeter. To maintain a certain deflection of the pointer under the action of a given controlling force requires a certain expenditure of energy, quite independently of the gauge of wire employed in winding the bobbin of the instrument, and this expenditure of energy produces necessarily a certain amount of heating. In neither instrument does this heating, unless very excessive, produce any effect on the strength of the current necessary to be maintained to produce a given deflection; but as the voltmeter is used to measure, not the current, but the difference of potentials necessary to be maintained at its terminals to produce this current, it follows that a change in the resistance of its bobbin, arising from the heating, must change its constant. Hence the amount of energy expended in a voltmeter must be less than that expended in an ammeter, or, in other words, the strength of its controlling magnet must be less.

Professor S. P. THOMPSON: My ammeter is much more constant than my voltmeter: I suppose for that reason.

Professor AYRTON: Yes, for that reason.

Professor D. E. HUGHES: I have not much experience with measuring instruments for large currents, but will offer a few remarks relative to the retention of magnetism by steel permanent magnets.

I have shown in my several papers upon the theory of magnetism, that vibrations have a great effect in reducing the apparent polarity to a state of neutrality. We can do so perfectly with all soft irons; and all steels, however hard, lose a certain proportion of their power when struck by a wooden mallet, or vibrated by any mechanical means. The most perfect retentive force would then be one which can resist this influence in the greatest degree. Steel magnets generally lose 50 per cent. of their magnetism by this treatment: some poor steels lose 75 per cent., the highest that I have yet found losing but 15 per cent. Now, as it is most important that we should know the retentive power of any magnet, particularly those of marine compasses, I have suggested that they should be first magnetised to a known degree, then vibrated by striking gentle blows with a wooden mallet, and then a second reading of its force to be taken. If we determine upon any standard, say, only 25 per cent. less, then all magnets or compass needles that lost 35 or 50 per cent. should be rejected as not suitable for the purpose intended. It is true that a magnet does not in ordinary use receive such violent treatment as I propose, but they are subjected to constant small mechanical vibrations, the sum of which in time gradually reduces its power exactly as we can do in a much shorter time by a more violent vibration. I employ this method myself whenever I wish to test the retentive power of a fine piece of steel, and above all whenever I make use of a magnet, as in my magnetic balance, whose force I wish to remain a constant for several years, and through all reasonable treatment.

Mr. C. E. SPAGNOLETTI: I have not had much experience in measuring instruments for very strong currents, but can say something in reference to the constancy of magnets.

Some years ago I made an induced magnetic needle to withstand the attacks of lightning, to be employed in the ordinary needle speaking instrument or needle "block" instrument. There are now some 80,000 of these instruments, with two magnets in each, in use, and I find after years of work that the power from the magnets which have no armatures is very good and still constant.

I have not taken any delicate measurements of their retentive power, but the battery power has not been increased; and in some instruments the magnets have been working constantly for 12 to 14 years without having been touched, and there is no visible difference in the strength of the signals, although one would think that the occasional effects of lightning which we have through the coils might help to weaken the magnets, as might also the working currents which are constantly being sent round them.

What I did to obtain these results was, to divide the axle and the needle by brazing in a piece of brass in the centre, which enabled me to get a straight magnetic needle between the magnets, which is placed in such a position that the axle was magnetised by the two norths on one side and by the two souths on the other. Now, whether or not any saving of the power is gained by placing the magnets north to north and south to south, so as to establish a magnetic equilibrium in any way, I do not know, but the magnets have been most constant in their working, as I say, for some 12 to 14 years, and have and continue to do good service and economise labour and battery power.

As regards Mr. Crompton's measuring instrument, I was trying to get an instrument something of the same kind, with the divided needle I have just described, but my idea was to have a second coil in the same circuit, so that the forces of the needle to be affected and the magnet affecting them should be as nearly as possible the same. I have found with some instruments that with two magnets of varying strength, or with electro and permanent magnets put close together, the weaker one is overpowered by the stronger.

I tried the effect of surrounding a small soft iron needle by a

wire, taking its strength by the same current that is passing through the outer electro-magnet, and should like to know whether Mr. Crompton has tried that or a similar plan at all in his experiments, and if so, with what result.

The PRESIDENT: In addition to the method of control adopted by Mr. Crompton and Mr. Kapp, we have had described the methods which are employed in controlling measuring instruments by means of a spring, as in Professors Ayrton and Perry's new instruments, and by means of a magnet, as in Sir William Thomson's instruments. In regard to what fell from Dr. Hopkinson, I should like to say that I have used both of Sir William Thomson's instruments, and, referring specially to the one for measuring differences of potential, I have found that the value of the constant at the time when I was using it was not the value sent with the instrument—*i.e.*, the magnet had in some way or other had its strength altered since it was tested in Glasgow. During the time that I had it in use, throughout a considerable interval, there seemed to be no change at all in the strength of the magnetism: the change seemed to have taken place previously, possibly from the knocking about which it had had. After the first Glasgow tests were made I measured the value at the time, and it kept pretty constant during the series of experiments; still it is always a matter of uncertainty whether these instruments remain constant or not, and in such cases it is necessary to have the means of measuring from time to time what the value of the magnetic field is, so as to know the absolute value of the measurements which are made.

In these instruments Mr. Crompton and Mr. Kapp have adopted the principle of controlling by an electro-magnet, and I am sure every one must admire the simplicity of these instruments, especially the current-meter. The simplicity is such that no one can have any difficulty in seeing through the whole principle of the apparatus; in fact the principle is seen at a glance, and this constitutes no small merit in measuring instruments of this class. We have now the opportunity of testing these, and comparing them with other instruments, and no doubt before long we shall hear more as to the way in which they do the work for which

they are designed. I will now ask Mr. Crompton to reply to the points which have been raised in the discussion.

Mr. R. E. CROMPTON: Mr. President and gentlemen,—You will perhaps observe that in the body of our paper I have abstained as far as possible from comparing our new and comparatively untried instruments with those of our predecessors, whose instruments have been well tried by many of you. Our instruments will soon be in every one's hands, and I hope that they will show great advantages which will render them of high scientific and commercial value.

I cannot refrain from attacking Sir William Thomson, but he is so great a man that he will forgive me if I, as a pigmy, do so. Sir William Thomson's instruments were the starting-point for us. We have a very fine instrument from him in our works, which is used as our standard instrument, and it was the necessity for frequent recalibration of this instrument, which gave us so much trouble, that caused Mr. Kapp and myself to commence the series of experiments which has resulted in these new instruments. One or other of the two magnets of Sir William's instrument was always on its way backwards and forwards between Mr. White's at Glasgow and our works. The difficulty with us is, that our works are a manufactory of dynamos, which are spread all over the place. We have built an observatory at some distance from the shops, but it would be an intolerable inconvenience to have to make all our observations at that great distance from the dynamos. No amount of speaking tubes or telephone apparatus will make up for the convenience of having your instrument near your dynamos. At an electric lighting station the instruments *must* be near the dynamo-room. We have used the three best instruments in the market,—Sir William Thomson's, Professors Ayrton and Perry's permanent magnet instruments, and Siemens' dynamometer,—and although they are all good instruments they are all liable to change of calibration. This change can be tested by us, no doubt, and can be corrected from time to time, but such corrections are not possible to the people into whose hands the instrument falls. In all these early instruments there can be no doubt that the necessity for frequent recalibration is greatly

increased when they are used in close proximity to dynamos, or even to steam or gas-engines in motion.

I will take together all the remarks made on the subject of springs. The question of the permanency of springs of course is a most interesting one, and I am disposed to agree with those gentlemen who have spoken, that springs, provided they are not overstrained, are practically permanent enough for our purposes. It is just this likelihood that they *will* be overstrained by accidental shocks when being moved from place to place, or when travelling, and the want of visible evidence that such overstraining has taken place, that makes their use so objectionable. I have used many instruments which depend upon springs, and many of them (twenty or so) have became untrustworthy from the springs being overstrained in most cases from a fall or jar in the railway, or some such cause. This is one difficulty I have with regard to springs; but there is another. Take Siemens' dynamometer, for instance, and place it in a battery room, or wherever there are oxidising vapours present, and you know that from oxidation alone the sectional area of the spring of the instrument must be reduced in time, and consequently the spring will become weakened. Further, let us compare the cost of a good spring with that of our arrangement of electro-magnets. After all it is the matter of cost, or, in other words, of facility of manufacture, that we must consider; and I believe that a carefully calibrated spring is a far more delicate and expensive article to reproduce on a commercial scale than our arrangement of two coils of wire, with two pieces of charcoal iron wire pushed through the centre of them.

Curiously enough I am accidentally able to answer Professor Silvanus Thompson's remark as to the effects of high pressure steam upon springs. Long before I had anything to do with electricity I was managing works in India where we used steam at 180 lbs. pressure, and spring-loaded safety valves, and there is no doubt that those safety-valve springs weakened perceptibly, mouth after month, from the perpetual blowing past them of steam at the temperature of 180 lbs. Very few people have used such high pressure steam, and consequently such difficulties as

we had with those springs are not known generally to engineers who have never exceeded 140 lbs. pressure.

As to what Professor Ayrton said in regard to the use of the word "meter," there is nothing very important in what I brought forward; but the word "meter" is the legal term in use in the electric lighting provisional orders, and that word will be used, no doubt, for integrating instruments, and it will produce confusion in sending telegrams if used for other instruments. We at one time thought of using the word "gauge" to the potential indicator, because we wanted to describe to the engineering mind an instrument to indicate the pressure of the current, just as a steam-gauge indicates the pressure at the boiler.

In regard to the permanency of steel magnets, I think I have already answered this question for Sir William Thomson's instruments. The same thing has occurred to us as with every other kind of instrument we have used. No doubt it is much intensified in our works by the constant movement of masses of iron, and from the powerful electro-magnets that are in use. But I would point out this, that whereas it is probable, and in fact, I should say, certain, that permanent magnets are liable to be permanently weakened by being brought into proximity with these powerful electro-magnets, our instruments, although they may be slightly affected *at the time* (and it is certainly a very slight effect), are not *permanently* affected, because they contain nothing but soft iron. This is the difference we claim, and we prove it to our own minds by the fact that the first potential-indicator made by us, and calibrated at our works, is still our standard instrument. Sir William Thomson's instruments have erred and come back to it four or five times, although both instrument have been ill-used to an equal extent by being placed near the dynamos. Our instrument has been far more ill-used than Sir William's, for it has been placed almost on the top of the dynamos, whereas the others have always been kept twenty or thirty feet away, if possible.

The next point brought forward by Professor Ayrton was the question of dead-beatness. Now, our suspended instrument is fairly dead-beat, but the pivoted instrument is very dead-beat. He will appreciate it when I tell him that at my own house,

where I always have one, it follows every beat of the gas-engine, and registers the admission of the gas as regularly as one of his own instruments would do.

The question of the size of the instrument has not been mentioned. The instruments look clumsy, but the reason is that we must get in the large resistance coil plenty of cooling surface for potential indicators which are intended to have the current passing continuously through them. All our potential indicators are intended to be thus used, and I have not hitherto met with any potential indicator that would not heat up under such a continuous test. If the engine-driver is to drive his engine by the potential indicator, which is what we have all intended should be the case, he wants an indicator which can be kept on for eight or ten hours, as the case may be, and that necessitates a large instrument. Moreover, there is no call in our case for a small instrument: all the people ask for a dial as big as a clock, which can be seen some distance off. In the first instruments we made the mistake of making them too small.

I am afraid I did not quite follow Mr. Spagnoletti's remarks: perhaps he would be kind enough to explain them again. I did not quite understand whether it was that his second coil was to be placed in any sense as I explained in the body of my paper, viz., to have two coils at right angles to one another, or whether he represented his inner coil to be simply a deadening or damping coil.

Mr. C. E. Spagnoletti: The inner coil was to surround the magnet to put more magnetic force into it.——

Mr. R. E. Crompton: Needle itself?

Mr. C. E. Spagnoletti: Yes, so as to equalise the forces one against the other.

Mr. R. E. Crompton: I think that is distinct from anything that we have done. I think I have replied to all points; but the pith of the whole business in our case was to get an electro-magnet to work as the controlling force. We have succeeded in doing this, and I think, to our minds at least, we have proved that the instruments do not lose calibration. Some of our instruments have been in constant use for nearly a year, and those first

made are as exact as they were at first (or as inaccurate as they were at first): there is not the least change in calibration. Those that were calibrated with Sir William Thomson's instrument when it was inexact, have still retained that inexactness: those that we managed to get correct are still perfectly correct. There is no difficulty in working, and no uncertain jump in their deflection, which has been the great difficulty we met with at first in producing such instruments.

The PRESIDENT: I am sure that you will agree with me that we have had a very successful meeting this evening. So many interesting points were raised by the paper, that we have had a good discussion, and Mr. Crompton has just added some weighty and valuable remarks in closing the discussion. I ask you to give Mr. Crompton and Mr. Kapp a hearty vote of thanks for the interesting communication which they have brought before us.

The vote was unanimously accorded.

A ballot then took place, at which the following were elected :—

Associates:

J. C. Chambers. | William Lind.
J. Slater Lewis. | William Milner.
Michael Holroyd Smith.

Students:

David Abercrombie. | Frederick Bathurst. | C. H. Wordingham.

The meeting then adjourned until Thursday, 28th February, 1884.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY TO FEBRUARY 1, 1884.

By Alfred J. Frost, *Librarian.*

(*Works marked with an Asterisk* (*) *have been purchased. The Catalogue of the collection of Works from the library of the late* Sir Wm. Siemens, F.R.S., D.C.L., *presented by* Lady Siemens, *will be found at the end of this list.*)

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A. J. FROST,
Librarian.

STATEMENT OF RECEIPTS AND EXPENDITURE FOR THE YEAR 1883.

As received and adopted by the General Meeting, 28th February, 1884.

RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance in hands of Bankers, December, 31st, 1882	396	1	4			
„ Balance in hands of Secretary, December 31st, 1882	3	19	9			
				400	1	1
„ Subscriptions paid up for previous years ...				153	17	6
„ Do. for 1883				1,213	9	0
„ Do. in advance				28	8	6
„ Entrance Fees due 1882				5	4	0
„ Do. due 1883				149	2	0
„ Life Compositions				133	0	0
„ Publishing Fund				15	15	0
„ Sale of Journals and other Publications ...				149	19	7
„ Sale of Ronalds Catalogue				18	8	
„ Profit on Exchange				1	3	9
„ Dividends on Stock (Premium Fund)...	8	2	1			
„ Dividends on Stock (Life Compositions)	28	15	10			
				36	17	11
„ Advertisements in Journal, 1882				20	0	0
..., as shown above				287	12	0
				£2,995	9	9

EXPENDITURE.

	£	s.	d.	£	s.	d.
By Salaries and Clerical Assistance ...				445	0	0
„ Shorthand Reporter				18	0	0
„ Attendance, Refreshments, and other Expenses attending Evening Meetings				31	18	11
„ Printing and Illustrating Journal and cost of Abstracts and Advertising	422	11	4			
„ General Printing and Stationery	45	18	3			
				468	9	7
„ Insurance				5	0	0
„ Purchase of Books and Pamphlets	39	10	1			
„ Binding Books & Periodicals	18	12	4			
				58	2	5
„ Rent, Gas, and Firing				215	13	6
„ Postage of Journal, Notices of Meetings, &c., and General Office Expenses				149	17	7
„ Petty Expenses of Local Honorary Secretaries				1	16	6
„ Bank Charges on Foreign and Provincial Drafts, &c.				0	16	2
„ Premium Account—Society's Premium awarded to Mr. J. Munro, Associate				10	0	0
				£2,995	9	9

We certify that we have examined the Books, Vouchers, and Securities of the Society, and that the above Statement of Receipts and Expenditure and Estimate of Assets and Liabilities are correct and exhibit the true financial state and condition of the Society.

J. WAGSTAFF BLUNDELL (WAGSTAFF BLUNDELL, BIGGS, & CO., Chartered Accountants), FRED. CHAS. DANVERS } Auditors.

ORIGINAL COMMUNICATIONS.

ON THE DIRECTION ASSUMED BY A MAGNET WITHIN A SOLENOID OR HOLLOW MAGNET.

By S. M. BANKER.

I had an occasion recently to prepare a short description of the vertical galvanometer, or what is called by telegraph linemen the "detector." When in due course I came to number up the forces which held the needle in equilibrium, I discovered I had an effect which I failed to find noticed in works connected with the science of electricity.

The generally-accepted explanation of the galvanometer coil goes no further than to describe the action of a current traversing a wire in the neighbourhood of a magnet on that magnet, and then the multiplying power of the current on the magnet by bending the wire around the magnet so as to enclose it within a coil of the wire, called a bobbin.

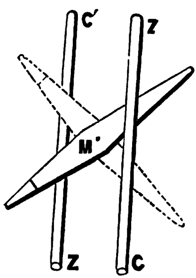


FIG. 1.

The explanation is as follows:—Suppose M to be a magnetic needle, axled through its centre and free to move in a vertical plane, with a wire placed vertically between the needle and the observer, as shown in Fig. 1. A current passing through this wire from C to Z would deflect the north end of the needle to the left hand; if, however, the needle be placed between the wire and the observer, and the current sent through the wire from C′ to Z′, the north end of the needle will be still deflected to the left hand, showing that a current passing up a wire in front of a magnetic needle has the same effect on it as a current passing down a wire when placed behind it; hence by joining the two wires at Z and

C′, the passing current will have twice the force acting on the needle. If we now add another piece of wire similar to C Z C′ Z′, and join the former's lower front end to Z′, the passing current would deflect the needle nearly fourfold compared with the action of the current when only passing through the wire C Z. By increasing the number of these pieces of wire, the force emanating from the current passing through the wire will be increased in the same ratio. The pieces of wire added above form a continuous wire around the magnetic needle in the shape of a spiral or helix, and by placing one spiral upon another we increase the magnetic effect of the current until we reach a point where the magnetic effect of a spiral is balanced by the increased resistance of the bobbin.

If in the above experiments the current is reversed, the deflection of the needle becomes reversed.

But we have another action, independent of the force producing the deflection of the needle, by bending the wire into a solenoid—that is, the same current which produces the deflection of the magnetic needle also imparts to the helix a property similar to a magnet, giving to it a polar force.* Hence it must be evident that the same current which produces a deflection of a magnetic needle inside a bobbin, also produces poles to the bobbin itself, exactly similar to a magnet. Moreover, it must also be evident that this force at the ends of the solenoid must have some influence on the magnetic needle placed within the bobbin.

We will now examine what influence this polarity of the spiral will have on the magnetic needle placed within it.

In twisting a wire into a spiral, there are two ways of performing the operation: one is called a right-handed spiral, similar to an ordinary screw thread, and the other a left-handed spiral. For illustration, I have taken the former in Fig. 2. A current entering at the right-hand, C, would make that end a south pole, and the left hand a north pole. If we reverse the direction of the current, we also reverse the polarity. Now the current entering at C makes that end south, and the other or left-hand side, Z,

* "Electricity and Magnetism," by Clerk Maxwell. Vol. II., pars. 396–9, 676–8.

north. This current will also ascend the coil-wires between the observer and the needle, and, from what has already been explained with respect to Fig. 1, shows that the north end of the magnetic needle will be deflected to the left. But the left-hand end of the spiral is its north end, therefore the north end of the magnetic needle goes to the north end of the spiral—north to north and south to south, an apparent paradox—a result at first sight we should hardly anticipate, but with a little consideration we see that it must be so.

In looking for the forces which affect the deflection of the magnetic needle in all galvanometers, we have not only to look for gravity, inertia, friction, etc., but also a repulsion between the poles of the bobbin and the poles of the magnetic needle.

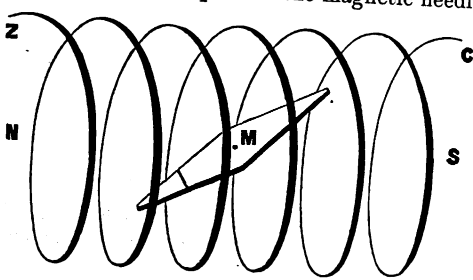


FIG. 2.

The fact appears to have been hitherto overlooked that a current passing through a bobbin of wire has a twofold action:—

1st. The action on the magnetic needle.

2nd. The polar action of the bobbin.

The experiment can be nicely shown by using a coil of wire sufficiently large to allow a horizontal magnetic needle to pass along its axis. It will be found that the needle will pass from any point outside the coil, through it, and out at the other side, without the least deviation towards either end of the coil.

This shows that a magnetic needle outside a coil of wire follows the usual law of north attracting south, and *vice versa*; but when within the coil this law is masked, and the current in the wire predominates.

If we carry the experiment still further, we shall find that the

same takes place when a magnet is placed within a hollow cylindrical magnet. Take a piece of thin iron and bend it into a cylinder of a size just sufficient to allow the coil of wire in Fig. 2 to slip over it—do this and put the current on in same direction as before, the needle will be deflected N. to N. and S. to S., as in the experiment before the iron cylinder was added. If the iron of the cylinder be hard, it will retain sufficient magnetism after the removal of the solenoid to act as a magnet, and it will still be found that the N. end of the interior magnet will be deflected to the N. end of the exterior magnet, and the same with the other end.

The same experiment may be made in this case as with this solenoid of wire, viz.:—Take a hollow cylindrical magnet, and a horizontal magnet sufficiently small to pass through the former, and after allowing the horizontal magnet to take up its position near one pole of the hollow magnet, it can then be passed along the axis of the hollow magnet from one side to the other without being in the least affected by the hollow magnet.

A DETERMINATION OF THE LOSS OF TIME ON THE CABLE FROM LOURENÇO MARQUES (DELAGOA BAY) TO DURBAN, AND ON THE LAND-LINE (WITH THREE RELAYS) FROM DURBAN TO THE CAPE OBSERVATORY.

By W. H. Finlay, B.A., F.R.A.S., Chief Assistant at the Royal Observatory, Cape of Good Hope, Associate.

This paper contains the results of observations made at the request of J. Sivewright, Esq., C.M.G., General Manager of South African Telegraphs.

On my arrival at Lourenço Marques in November, 1881, on my way back from Aden, I received a telegram from the Astronomer-Royal at the Cape, asking me to make preliminary trials and arrangements for signals. In these I met with most cordial assistance from Messrs. Heraghty and Carlisle, telegraph superintendents at Lourenço Marques and Durban respectively. It was found impossible to use the mirror galvanometer between

these stations, as a single cell sent the spot of light off the scale, so we determined to use the Morse, as in the ordinary working.

The battery consisted of 10 Leclanché cells. The signals were exchanged on the night of November 4, and the programme was as follows:—

(1) Signals were sent from the Cape Observatory (Obs.) by Mr. Pett at intervals of 10 seconds for 8 minutes, and the time of their arrival noted at Durban (Db.) by Mr. Maclear; then signals were sent from Db. to Obs. for 15 minutes; and lastly, signals again from Obs. to Db. for 8 minutes.

(2) A similar set of signals was interchanged between Db. and Lourenço Marques (L. M.).

(3) A Siemen's relay was inserted between the cable and land-line at Db., and signals interchanged between Obs. and L. M.

At the Cape a *sidereal* clock was used, at L. M. a *sidereal* half-seconds chronometer, and at Db. a *mean* time clock, which had been put up there for the Cape Longitude Expedition.

Now a sidereal clock gains 1 second in 6 minutes approximately on a mean time clock, so that once in every 6 minutes the Obs. and Db. clocks would be beating together, and once in 3 minutes the Db. clock and L. M. chronometer.

These coincidences were the phase observed in operations (1) and (2). In (3), the time of the signals being received had to be estimated to tenths of a second; but Mr. Pett and I have had long experience of such signals, and have always agreed (in the mean of several observations) in the subdivision of the seconds.

The absolute errors of the clocks are of no importance for my present purpose—all that is wanted is their rates during the time of the signals. For the rates at the Cape and Db. I am indebted to Mr. Gill, Astronomer-Royal at the Cape. The rate of my chronometer at L. M., I determined from observations there. The observations extended over a considerable part of the night, and I have not reduced all the signals to a common epoch; but each operation is complete in itself—*i.e.*, reduced for rate to its mean epoch.

The only other point to be considered is the personal equation of the different observers in sending and receiving signals. All

signals were sent in coincidence with the beats of the timepiece, and the accepted received signals in (1) and (2) were coincidences of the click of the lever and the clock-beat, so that not much error on this score should be found in the case of trained observers. Experiments, however, were made to determine this important point (cf. Mr. Gill's "Preliminary Account of the Determination of the Longitude of the Cape Observatory," *Monthly Notices, R.A.S.*, Vol. XLIII., No. 8), and the result was that the difference between any two of the observers, either in sending or receiving signals, or in a combination of the two, was less than 0·01 seconds. This personal equation may therefore be fairly neglected.

The length of the cable from L. M. to Db. is 349·7 knots; resistance of copper conductor 11·35 ohms per knot; capacity ·283 microfarads per knot; and the capacity of the condensers is 38·54 microfarads.*

The length of the land-line from the Obs. to Db. is 1,133 miles. Three relays were inserted, viz., at George, Fort Beaufort, and Umtata, so that to the resistance of the wire must be added the resistance of the instruments. The resistance of the whole line may be taken at a little more than 9 ohms per mile.†

The following are the results of the observations:—

(1) Db. fast on Obs.—

	H.	M.	S.	
Obs. sending	0	50	12·29	(2 coincidences).
Db. „	0	50	11·92	(2 „).

$$\therefore \text{loss of time} = \frac{0^{\text{s}}{\cdot}37}{2} = 0^{\text{s}}{\cdot}185.$$

(2) L. M. fast on Db.—

	H.	M.	S.	
Db. sending	5	52	39·89	(5 coincidences).
L. M. „	5	52	39·37	(2 „).

$$\therefore \text{loss of time} = \frac{0^{\text{s}}{\cdot}52}{2} = 0^{\text{s}}{\cdot}260.$$

* For this information I am indebted to the Eastern and South African Telegraph Company.

† For this information I am indebted to the Cape Government Telegraph Office.

(3) L. M. fast on Obs.

		H.	M.	S.
Obs. sending	... ...	6	42	54·24.
L. M. "	... ...	6	42	53·35.

$$\therefore \text{ loss of time } = \frac{0\overset{\text{s.}}{\cdot}89}{2} = 0\cdot445.$$

The exact agreement of the loss in operation (3), which is independent of the observer at Db., with the sum of those in (1) and (2), shows that the personal equation of the observers in these signals was practically insensible, as I stated before.

These results give for the cable a velocity of transmission of about 1,370 knots per second, and for the land-line of about 7,630 miles per second (through three relays).

There must, however, be some loss while the lever is moving from its position of rest to the lower stop. This distance was adjusted finely, and the antagonistic spring just sufficient to draw the lever back. I have no data at present to determine this time, but I do not think it would exceed 0·02 seconds.

If this estimate be accepted, we should have for the loss of time on the cable before the current made itself sensible 0·24 seconds, which corresponds to a velocity of transmission of about 1,460 knots per second.

ROYAL OBSERVATORY,
CAPE OF GOOD HOPE,
21st Nov., 1883.

ON THE CAUSES OF FAILURE OF DEEP-SEA CABLES.

By JAMES GRAVES.

The quarterly Journal of the Society (Vol. XII., No. 50) has just reached me, giving *in extenso* the paper of Messrs. Trott and Hamilton, and the discussion thereon upon the 29th November, 1883, and I am extremely interested therein, although the discussion did not develop the subject brought forward to any great extent, as the speakers appeared to doubt the facts laid before them by the authors.

Many references were made to the 1865 Atlantic cable, which,

on behalf of the Atlantic Telegraph Company (as the representative of Mr. C. F. Varley), I tested throughout from the commencement of the core manufacture to the completion of the shipment on board the "Great Eastern," and therefore took a most lively interest in its welfare, not only before it was laid, but afterwards in superintending its working until it died a natural or a suicidal death at about 9 a.m. on the 11th March, 1873, and to my great regret was never resuscitated.

This break, about 580 knots from Valentia, in 1875 fathoms, was followed almost immediately after, on 9th April, 1873, by another about 795 knots from Newfoundland, in 2,400 fathoms, and three years later, on 10th June, 1876, by yet another about 185 knots from Valentia, in 1,200 fathoms. This length remained intact until January, 1882, when it was broken during the repairs to the 1874 cable off the Skelligs. Meanwhile part of the western end of it had been picked up and utilised to form the whole of one and half another cable from Heart's Content across Trinity Bay to Rantem—one of which being termed the "Rantem" cable and the other the "Island Cove" cable, in order to distinguish them. These cables are laid chiefly in depths of 150 to 200 fathoms, and are about 60 miles long, and work admirably.

Such has been the fate of the cable of 1865. The type of this cable was so carefully considered before the specification was completed and adopted, and such diligent care was bestowed throughout its manufacture to secure a perfect cable, that it is worthy of the deepest consideration for the purpose of endeavouring to discover why its laying could not be completed during the summer of 1865, and why it should have failed after it was laid in several places in deep water; that being the type more or less adhered to in all the deep-sea cables since laid, aggregating many thousands of knots, and representing many millions of capital.

As the practical representative of the Atlantic Telegraph Company during the manufacture of this 1865 cable, and also their representative at Valentia during its submersion, as well as their already appointed superintendent at this station, I very naturally sought out the prevailing opinions of the leading men

engaged on the "Great Eastern," soon after her return to England, as to the cause of the failure, and from notes made at the time, bearing date 20th September, 1865, I select the following extracts:—

No. 1 was afraid the injuries were malicious.

No. 2 thought the paying-out system defective, as during half the time there was no check on the insulation. He was decidedly of opinion that the *first* and *second* faults were purely accidental, and upon seeing the first fault remarked, "We shall have plenty of these." He believed the iron wire to be brittle in some places, and to become broken during shipment, unnoticed. He saw one place where six inches of wire was missing from the hemp, and the ends protruding. In coiling down, these brittle scraps get between the flakes, and are squeezed into the cable, but may not penetrate the core until, in passing through the paying-out machinery, they get pressed into it. He suggested softer wire, even to the partial sacrifice of strength.

No. 3 believed faults malicious. He would have had the culprit lynched if he could have found him. He considered the picking-up gear too weak for such deep water. He would have no strangers engaged at the last moment before sailing. Sometimes the engines were not stopped, and at others were not started promptly upon the order being given, and extra strain was thus brought to bear on the cable.

No. 4 was on duty in the tank when the *third* fault occurred. He heard a scraping noise; saw a broken wire; called out, "A broken wire!" and soon after a fault was reported. He was of opinion the *second* fault was maliciously done, as one end of the wire was partially pointed by filing, and the other looked as if filed partially through and then snapped off.

No. 5 was of opinion the first two faults were caused maliciously. He examined the piece of wire from the *second* fault with a lens, and was convinced that one end had been tapered by grinding, and not by filing, and that the other end had been nicked with a file at a length equal to the diameter of the cable, and that this had been forced into the cable and then broken off.

No. 6 believed both faults were malicious, and that they could not possibly be accidental.

No. 7 firmly believed the faults to be malicious.

No. 8 thought the *second* fault was done maliciously. He recommended that no strange men be allowed on board, and none but well-tried factory men be employed. He saw several strange faces, though he was himself an old employé.

The *third* fault was never recovered, but there is no doubt that the broken wire seen by No. 4 had something to do with it.

These extracts are given here merely to show the current feeling at the time of the 1865 failure amongst those who were actually engaged upon the work, after both officers and men had had a month or six weeks to ruminate over the bare facts and draw conclusions therefrom.

The most probable causes of these three faults were those given by No. 2, as it is easily imaginable that out of 20,000 miles of wire a few brittle short pieces should exist, fall out of the hempen strands, and bury themselves in the tank; the superincumbent weight squeezing them into the covering of the cable, and then upon passing through the machinery the mischief was finished. By the time the tests showed the fact, and the electricians could verify them before giving the alarm, 10½ knots in one case and 2½ in another had been paid out. Hence, assuming the wire with which the cable was "protected" to be the real "culprit," there is but the one conclusion, that it alone was the cause of the failure to lay this cable successfully in the year 1865; and this led me to suggest, in a letter to the Secretary of the Atlantic Telegraph Company, the total abolition of iron in the covering of the next cable.

Now comes the question, why this cable broke in deep water in several places after it had been completed and worked for several years.

I know the usual assumption that the cable was suspended in catenary curves from peak to peak, as referred to by Mr. Forde (p. 513), owing to inequalities in depth within short distances, and so got broken when sufficiently weakened; but has this been sufficiently proved in relation to this 1865 cable, or is it still mere

assumption? because the latter would be poor evidence upon such a vital question, however easy it may be to get out of a difficulty by such an assumption. Should we not rather fall in with the views of Captain Trott and Mr. Hamilton, and attribute these failures in deep water to the "wringing" phenomenon developed by their experience, and brought to bear upon the weak places in the cable?

Professor Fleeming Jenkin, in his Cantor lecture at the Society of Arts, February 5th, 1866, laid great stress upon the protective nature of a completely closed cable sheathing, and showed that it could not stretch because it formed a solid tubing round the core; and when alluding to the Malta-Alexandria cable, he remarked that "no sensible untwisting ever does occur; about 40 or 50 turns are, at most, taken out per mile." And further, referring to the 1865 cable, he says it "is the strongest cable yet made, bearing more than twice as great a length of itself as the old iron cable. The new form stretches more than the old. The hemp may be eaten off, or decay from the wires, weakening the cable, and the hemp affords less mechanical protection against injury, but the stretch is never such as to endanger the core, as has been proved by repeated experiments."

The twisting (or untwisting) of the cable in paying out, although in theory and experiment said to be not "sensible," has, however, been observed by Messrs. Trott and Hamilton in actual practice. To be visible between the stern of the ship and the surface of the water it is fair to assume that *two* turns would have to be made to attract notice, and that on a length of probably not more than 20 fathoms.

Now, if two turns are taken out in 20 fathoms, and this action is continued in the same ratio to the bottom, at a distance of 2,000 fathoms, it would amount to 200 turns: how much more would it be when, as Mr. Forde remarked (p. 513), in such a depth there are frequently "20 or 30 miles of cable probably suspended in the water from the stern of the ship to where the cable touches the bottom"?

Or, to take the more modest estimate of Professor F. Jenkin, say, 30 turns per mile (50 being the possible maximum), and

applying his further estimate, that in 2,000 fathoms, the cable being laid from the ship at an angle of 9° 30′ (average) and a strain of 12 cwt. (average), there are from 12 to 13 miles off the bottom, 30 turns per mile for 12 miles would yield 360 turns.

Our late President said that he "could not say how many twists a length of core would stand before breaking, because he had never had patience to go far enough; but he had twisted it 50 times without obtaining a break." This was, as a matter of course, tried upon *new* core. What would be the probable effect of 360 or more turns on *old* core?

As regards the protecting power of the iron, it is well known that it does *not* form a solid tube round the core, so as to resist the crushing power of a heavy strain in the Atlantic type. The diameter of the steel wires (homogeneous) is 0·095 inch, and these, being 10 in number, would form a circle whose circumference would measure (through the centre of the wires) 0·95 inch; and as the diameter of the core is 0·464, its circumference would be 1·45 inch, leaving a difference of half an inch in the event of all the hemp and jute being destroyed and removed. The ten wires at their full gauge, when new, would not surround the core in a complete helical tube, and any twist or strain would tend to cause them to cut into the core and damage it.

In paying out a cable, the twists are spread out and kept stretched as long as a steady strain is kept upon the cable, and are distributed along its length; but if the ship stops, and this strain is relieved and a lot of slack runs out, as it will do, a reaction sets in, and the twists which have been taken out of the length remaining off the bottom immediately endeavour to recover themselves, and expend their force upon the slack at the bottom, and form hoops, ready to be drawn into kinks the moment a strain is put upon them in the attempt to pick up, and then a fracture results. Every one knows how much easier it is to break a piece of wire by putting a kink in it, than by fairly straining it. Hence the cable breaks "at the bottom," and "not near the ship," "when" (p. 528) "the ship is still and the cable hanging perpendicular."

It appears to me that this can only be avoided by keeping a

permanent strain upon the cable, as in paying out, but in practice this is too difficult a thing to ever hope to do on board a ship.

Again, as regards the twisting effect in picking up a cable in deep water, these twists, or rather untwists, are driven from the bow-sheave towards the bottom. If the cable could be hauled in hand over hand by manual labour, without any pressure upon it, the cable might stand a chance of coming up with its lay intact, but in practice this is impossible. The cable lies in the sheave, and the depth of water and the specific gravity of the cable regulate the pressure between the cable and the groove of the sheave. This pressure is sometimes very great. Captain Trott remarked (p. 527), "I have seen the cable come up quite flat over the sheave, the lay much elongated, and the core spewing out." This pressure makes the section of the cable a flattened oval, instead of a circle; the lay cannot pass this great pressure into the ship as it should do if no turns were to be taken out; consequently the lay is lengthened considerably by the pressure and the strain of hauling it in by steam power over the sheave, and thus the turns taken out of the lay on the sheave are continually forced away from the ship towards the bottom, or to the weakest "link in the chain" off the bottom, and there expend their force (possibly amounting to several hundred turns in all) upon the weak spot, to the wrenching asunder of the fabric which, as Mr. Hamilton said (p. 531), "occurs invariably when cables are picked up in deep water." Of course he refers to cables crippled by age, as new cables containing their full element of strength have been known to be picked up without such accidents.

The most feasible explanation of the failures in deep water of submarine cables is, that from some geological cause or other at the bottom of the ocean the iron wires become weakened by oxidation, the hemp as a consequence becomes destroyed, and that the accumulated twists on both sides of a weak place concentrate their force upon it and wring the cable asunder, and that these failures are not necessarily due to abrasion on elevated ridges.

If therefore a cable can be made, such as has been suggested by Captain Trott and Mr. Hamilton, without twists,—one which "*cannot* twist" (p. 526),—there is some hope that such accidents and failures may be avoided in the future.

VALENTIA, *February* 12, 1884.

ON LIGHTNING AND ITS EFFECTS.

By JAS. GRAVES.

On the 21st July, 1878, between the hours of 3.30 and 4.30 p.m., I was watching the flashes of lightning during a thunderstorm, with three or four other persons from the Valentia cable station.

The storm was raging to the southward of our position, and appeared to be distant from 6 to 8 miles, estimated upon the velocity of sound taken at 5 seconds per mile.

In the direct line of the storm two prominent mountain summits, having an altitude of 1,033 and 1,300 feet respectively, stood out boldly between us and the storm-cloud, and upon or behind these elevated points the lightning appeared to expend itself.

As will be seen by Fig. 1, some of the flashes were very peculiar. They are numbered in the order of their occurrence.

No. 1 was the most vivid, and consisted of a vertical line with a zigzag centre.

No. 2 had the appearance of a silver fringe on the outline of a cloud, being composed of a series of curves. It was a fine line backed by a dark cloud, and its motion easily apparent. It was followed by a heavy downpour where we were, although the sun was shining brilliantly just before it.

No. 3 was a diagonal line with a semicircular centre.

No. 4 was vertical, with a peculiar kind of twittering in the lower half.

No. 5 and No. 6 were simultaneous, nearly straight, and broader at bottom than at top.

No. 7 formed a diagonal line, with a large zigzag in the centre.

Nos. 1, 2, 3, and 7 appeared first at an altitude of about 45°, and distinctly *descended*, whilst Nos. 4, 5, and 6 as distinctly *ascended* to an altitude of about 30°, and then tapered to a point and disappeared. All present agreed that these three flashes did *ascend*.

Next day news was received that three cows had been killed by one of these flashes, probably No. 1, as it descended vertically in an exact line from this station to the field, 6 miles distant, where the cows were killed; and I was informed that they were so burnt that their flesh became black, and that the stench was so intolerable that the farmer could not remove their hides.

On the 25th January, 1884, at a few minutes past 7 p.m., a thunderstorm broke over Valentia during a violent gale. There were only two flashes of lightning worthy of notice. One was distant about 4 miles, and the other was nearly overhead, and not more than half a mile off. The latter set fire to some timber

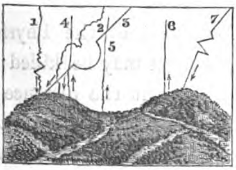


FIG. 1.


FIG. 2.

surrounding the engine-house chimney in the Slate Company's yard, distant an eighth of a mile from here, and also fused the coils of one of our Wheatstone receivers.

In connection with this flash a remarkable incident occurred. Although the discharge from the cloud took place within half a mile of this station, and some of its immediate effects were visible by the burning wood an eighth of a mile distant, yet a cottage situated on the roadside, about half-way up the side of a mountain whose summit is 877 feet above sea-level, and distant $1\frac{3}{4}$ miles in a direct line from the chimney upon which the flash expended itself, felt its effects severely.

A rough sketch of this cottage is given in Fig. 2. It had a slated roof, and the following are the details obtained from the occupants who were inside it at the time:—

The man, his wife, and some four or five children were sitting

round a turf fire on the hearth, when they felt a sensation as if the ground was going away from under them. The man was apparently temporarily stunned, for he remembered no more until he saw the stars shining through the roof. The woman, with a mother's instinct, seized the youngest child and rushed from the house into an adjoining field, the other children remaining inside.

A cow and a calf inside the cottage, at the gable end farthest from the fire, were both killed, and a pig outside the house was also killed: a large stone near the attic window in the gable was displaced, and the roof entirely destroyed, the rafters being broken and the slates thrown off from the gable to the chimney: the crockery on the dresser was broken to pieces, and other articles were displaced. None of the inmates remember seeing a flash, and not a sign of fire or scorching is visible anywhere. The cowhouse adjoining was not damaged in the least.

Mrs. Mary Somerville, in her "Connexion of the Physical Sciences" (1846, p. 315), remarks: "A person may be killed by lightning, although the explosion takes place at the distance of twenty miles, by what is called the back stroke. . . . Though the back stroke is often sufficiently powerful to destroy life, it is never so terrible in its effects as the direct shock."

The foregoing appears to be clearly a case of back stroke.

VALENTIA, *February* 14, 1884.

AN INVESTIGATION OF THE RELATIONS EXISTING AT RAPID RATES OF SIGNALLING BETWEEN THE EFFECTS OF THE ORIGINAL CURRENTS AND THOSE OF THE RESULTING EXTRA-RETARDING CURRENTS.

By FREDERICK KINSMAN,
Indian Government Telegraph Department.

By retardation of signals is meant, that with a given line wire with a battery at one end, and a receiver consisting of a soft iron core surrounded by coils of insulated copper wire at the other, if a current from the battery is set up for a definite time

in the line and receiver and then stopped, the core of the receiver will act as a magnet for some time after such cessation of the original current. This extra magnetic force may be due to one or all of the following causes :—

(*a*) That the core, if free, would take a certain time to demagnetise or lose its charge, such time of discharge being dependent on the quality of the soft iron.

(*b*) That on the cessation of the original current acting for a given time, a self-induced current is set up in the coils, the duration of such currents being dependent on the combined resistance and magnetic capacity of the receiver and the resistance and electrostatic capacity of the line.

(*c*) That a magneto-electric current is set up in the coils whilst the core is losing its magnetism, its duration also being dependent on the combined capacity and resistance of the receiver and line.

(*d*) That on the cessation of the original current a discharge takes place dependent on the electrostatic capacity of the line, the duration of such discharge being dependent on the same conditions as in (*a*) and (*b*).

(*e*) That on the cessation of the original current a self-induced current is set up in the line wire.

The direction of all the currents due to the above causes is the same, viz., that of the original current. Since causes (*a*), (*b*), and (*c*) are inseparable for any particular receiver, let their combined effect be called R, or receiver retardation; similarly let the combined effect of (*d*) and (*e*) be called L, or line retardation.

Now, take as above a line with a given static capacity and resistance, a core of given mass of soft iron and coils of given mass of copper, and a battery setting up through this line and receiver a current A, then, dependent on the static capacity and resistance of the line and the magnetic capacity and resistance of the receiver, both the line and receiver will take an appreciable time to get fully charged by the current A.

Let currents A be set up in the line and receiver for times B, and let x be the effect due to any of such original currents A acting for the times B, then the following cases may occur :—

I. When the combined capacity and resistance of the line and

of the receiver may be such that neither line nor receiver is fully charged by the first current A during time B, so that neither L nor R reach the maximum value obtainable with the current A, then L and R may be considered directly proportional to x for a time equal to or less than B that A may be effective.

II. Where one may be considered fully charged but not the other, then the effect of one kind of retardation will be constant whilst the other remains proportional to x.

III. Where both line and coils get fully charged, when both L and R become constants.

The condition necessary for a high rate of signalling is, that when rapid equal reversals, such as those produced by the Wheatstone automatic transmitter, are set up in a circuit containing a receiver, then the received signals shall also be equal. The matter for consideration therefore is, under what conditions do above cases, I., II., and III., admit of such a result?

In case I., let M be the total marking effect, and S the total spacing effect of equal marking and spacing currents set up in a given line and receiver, and, as before, let x be the effect due to the original current; let $L = n_2 x$, and $R = n_1 x$; let a marking current be first set up,

then $$M_1 = x + n_1 x + n_2 x = x(1 + n_1 + n_2).$$

Let $$n_1 + n_2 = n,$$

then

$$M_1 = x(1 + n), \text{ and } S_1 = x - nx + n(x - nx) = x(1 - n^2);$$

$$M_2 = x(1 + n^3), \text{ and } S_2 = x(1 - n^4);$$

generally $M_m = x(1 + n^{2m-1})$, and $S_m = x(1 - n^{2m})$.

Here the general expression for M and S shows that although the effects produced will never theoretically be equal, yet if n be given a fractional value, both M and S tend to the same limit, viz., x; and if n be small enough this limit will be approached so rapidly that, after a few reversals, M and S are practically both equal to x, and the condition necessary for high speed is obtained. If n is equal to or greater than unity, so that L and R are equal to or greater than x, then M and S no longer approach the same limit, and defective signals would be produced. Now the value

of $n = n_1 + n_2$ for a given current for a given time is, as shown above, dependent on the combined resistance and magnetic capacity of the receiver and resistance and electrostatic capacity of the line; also for a given line and a receiver of core of constant mass of soft iron surrounded by a constant mass of copper, the magnetic capacity of the core being constant, the value of n_1 may be considered directly proportional to the resistance, or, in other words, to the number of convolutions the copper is made to take round the core; and as regards the line, since both its electrostatic capacity and resistance are proportional to its length for a constant gauge of wire, n_2, with a given receiver, may be considered directly proportional to the length of line wire in circuit. This leads to the consideration, that with a line of given length and receiver of core of constant mass and dimensions of soft iron and coils of constant mass of copper, if the effect of the extra currents are proportional to that of the original currents, and their joint effect be less than that of the first original current sent, then, since the electrostatic capacity and resistance of the line and the magnetic capacity of the core will be constant, the fewer the number of convolutions the greater the speed obtainable. Case I. presupposes a high rate of speed, for an ordinary land line of about 350 miles in length will take less than ·002 seconds to get fully charged; hence a signal lasting ·006 seconds would admit of the line getting fully charged and discharged, and this length of signal corresponds to about 200 words per minute.

In case II., let C represent the effect of the constant retardation, and n the proportion between x and the variable retardation, then, taking M and S as before,

$$M_1 = x + n\,x + t^2 = x\,(1 + n) + t^2 \text{ and } S_1 = x - n\,x - t + n\,(x - n\,x - C) + C = x\,(1 - n^2) + n\,C;$$

$$M_2 = x\,(1 + n^3) + n^2\,t^2, \text{ and } S_2 = x\,(1 - n^4) + n^3\,C;$$

generally

$$M_m = x\,(1 + n^{2m-1}) + n^{2m2}\,C, \text{ and } S_m = x\,(1 - n^{2m}) + n^{2m-1}\,C.$$

Here, in order that the general expressions for M and S should tend to the same limit, viz., x, it is only necessary that n should be a fraction—*i.e.*, that the effect of the variable retardation

should be less than that of the first original current sent, for the constant retardation, C, still tends to disappear, even though it have a greater value than x.

Now case I. shows that with a given line and receiver, if the speed is such that line and receiver are not fully charged by the original current, then if n be greater than unity, perfect signals are not obtainable. Case II. points out what steps should be taken in order to again obtain perfect signals with the same given line where one of the effects R or L is less than and proportional to x; for in this case all that is necessary is that the other effect should be made constant, and this can be done in two ways, viz.—(*a*) by lengthening the duration of the reversals; (*b*) by reducing the value of R or L, whichever it is proposed to make constant.

As an example, suppose a given receiver and line working perfectly at a given speed under the conditions stated in case I., now increase the length of the line until the signals are no longer perfect, then perfect signals can be again obtained—

(*a*) By reducing the speed until either the receiver or line is fully charged; provided always the effect of the retardation due to the one not fully charged is less than the effect of the original current.

(*b*) Taking the receiver as of constant core and mass of copper, by reducing the resistance of the coils until the receiver gets fully charged by the original current; provided always the effect of the line retardation is less than the effect of the original current.

Generally, taking core and mass of copper of the receiver and gauge of line wire as constant. Increasing the length of line wire means reduced speed, or a receiver of less resistance and sensibility, requiring greater battery power.

Similarly increasing the resistance of the receiver necessitates reducing the speed or the length of the line.

In case III., where both R and L are constants, let $R = C_1$, and $L = C_2$, then, taking M and S as before,

$$M_1 = x + c_1 + c_2;\ \text{let } c_1 + c_2 = c,$$

then

$$M_1 = x + c, \text{ and } S_1 = x - c + c = x;$$

$$M_2 = x - c + c = x, \text{ and } S_2 = x - c + c = x;$$

generally

$$M_m = x, \text{ and } S_m = x.$$

In case II. it was shown, that with a given line and receiver, if the speed be such that one of the effects, L and R, be constant, and the other equal to or greater than x and proportional to it, then perfect signals are not obtainable. Case III. shows that with the same given line perfect signals will again be obtained if both L and R are made constant, for then every signal after the M_1 will equal x.

As in case II., the required result can be obtained by reducing either the speed or the resistance of the coils until both line and receiver get fully charged.

In this case, both L and R may exceed the duration of x without preventing perfect signals being obtained, but this result is obtained at the cost of a further reduction either in the speed or the number of convolutions of copper round the core.

Reasoning similar to that employed in the above three cases would show, that taking the coils of the receiver as constant, it is advantageous for high speed to reduce the size of the core.

Hence, within limits, the fewer the number of convolutions of insulated copper wire, and the smaller the core of a receiver, the greater the speed obtainable on a given line.

The reduction of the resistance of the coils is, however, limited by the fact, that the fewer the number of convolutions the greater the battery power required, whilst the reduction of the size of the core is limited by the amount of work it is required to do to make signals.

29th December, 1883.

ABSTRACTS.

Dr. E. DORN—REVIEW OF THE ELECTRICAL MEASUREMENTS MADE AT THE MUNICH EXHIBITION, 1882.

(*Elektrotechnische Zeitschrift, B. IV., No.* 10, *October* 1883, *p.* 404.)

The author, who was one of the Committee appointed to make the tests of the apparatus exhibited, gives an abstract of the official report published by the authorities of the Exhibition.

The apparatus used is first enumerated. One of Von Hefner Alteneck's transmission dynamometers was employed to measure the power transmitted by the driving-belt to the dynamo machines, and gave most satisfactory results. The speeds were determined by one of Buss and Sombart's tachometers.

All experiments were made in the three rooms set apart for the purpose from which four stout conductors of copper led to the machine-room.

The total resistance which could be used in the external circuit of any machine amounted in all to 272 Siemens' units. This resistance was made up of about 18 kilomètres of various wires stretched on insulators. There was (1st) a copper wire of 5·6 mm. diameter, and a total resistance of 1 S. U. divided into 0·1, 0·2, 0·2, and 0·5; (2nd) a copper wire of 3 mm. diameter, with a resistance of 1 S. U.; (3rd) an iron wire of 20 S. U. total resistance divided into 1, 2, 2, 5, 5, 5 units; (4th) an iron wire of 3·08 mm. diameter, having a resistance of 100 S. U.; and (5th) an iron wire of 150 S. U. resistance, which could be inserted by means of a commutator and served as a safeguard against any excessive current which might have damaged the instruments used for the measurements. The measurements of resistances were made by means of a large bridge of Siemens and Halske, and in a few instances by one of Beetz's universal compensators. The currents were measured by means of a Wiedemann's reflecting galvanometer, which was shunted by a copper wire 6·46 mm. in diameter, and which allowed of the measurement of currents varying between ½ and 170 ampères. Besides this principal instrument, two Siemens' electro-dynamometers and a Deprez's galvanometer were employed. Differences of potential were measured by one of Siemens' torsion galvanometers.

The units of measurements were previously determined by a sub-committee. The Siemens' unit was taken as being equal to 0·95 ohm, and the ampère as that current which would deposit 19·7 milligrammes of copper per minute; the volt naturally equals 1 ohm × 1 ampère.

The Wiedemann's galvanometer was very accurately calibrated by passing a current of about ½ to 1½ ampère through it and through a copper voltameter in series, and the other current-measuring instruments were compared with

it. The constant of the torsion galvanometer was ascertained by joining it up to the two ends of the copper resistance wire of 1 S. U., mentioned above, through which a current, accurately measured on the mirror galvanometer, was made to circulate.

The dynamos tested were arranged in two groups—series dynamos and shunt dynamos. In the case of the former, the resistances of the helix, R_1, of the electro-magnets, R_2, and of the whole machine, R, were measured, as well as the current, I, and the difference of potential, e, at the terminals; then the external resistance, $r, = \frac{e}{I}$, and the E.M.F. of the machine, $E, = e + R\,I$.

The total electrical work of the dynamo is then

$$L = E\,I \text{ watts} = \frac{E\,I}{9{\cdot}81} \text{ kilogrammètres per second,}$$

$$\text{or } L = \frac{E\,I}{746} \text{ English H.P.}$$

The useful electrical work in the outer circuit is

$$l = e\,I \text{ watts} = \frac{e\,I}{746} \text{ English H.P.}$$

If A is the power given to the dynamo as measured by the dynamometer, then $\frac{l}{L}$ is the electrical efficiency and $\frac{l}{A}$ the mechanical efficiency.

With shunt dynamos there were measured R_1, the resistance of the helix; R_2, the resistance of the electro-magnets; I, the current in the outer circuit; and e, the difference of potential at the terminals. Then if I_1 is the current in the helix, and I_2 that in the electro-magnets,

$$I_2 = \frac{e}{R_2}, \quad I_1 = I + I_2, \quad E = e + I_1 R_1.$$

The total electrical work is

$$L = I\,e + I_1^2 R_1 + I_2^2 R_2.$$

The electrical work absorbed in the arc lamps was measured at once by determining the current, I, passing through the lamp, and the difference of potential, ϵ, at its terminals.

From the observations on shunt dynamos it may be deduced that the effective magnetism is very nearly a linear function of the current in the electro magnets, or

$$\frac{E}{n} = \zeta + \eta\,I_2$$

Since

$$I = \frac{E\,R_2}{R_1 R_2 + R_1 r + R_2 r},$$

and

$$I_2 = I \frac{r}{R_2},$$

$$I = \frac{n\,\zeta\,R_2}{R_1 R_2 + (R_1 + R_2 - n\,\eta)\,r}.$$

All the results obtained are exhibited in an exhaustive table, which space does not allow of reproducing.

ANON.—SECONDARY BATTERIES AT THE VIENNA EXHIBITION.

(*Elektrotechnische Zeitschrift, B. IV., No.* 10, *October*, 1883, *p.* 418.)

One of the first exhibits likely to attract the visitor's attention was that of Planté, showing as it did the gradual development of the Planté cell since 1868.

The secondary batteries exhibited in the boiler-house by the International Company resembled very much those of Tommasi. Two square frames of lead strips, 6 cm. wide and 0·5 cm. thick, are placed in a wooden box. Between the two vertical sides of each frame are then placed a great number of thin laminæ of lead, which apparently are covered with minium.

An accumulator of Faure-Sellon-Volkmar cells was used for lighting several dwelling-rooms by means of Swan lamps. The Emperor's pavilion, for instance, was lighted by 48 Swan lamps, each taking 1·5 ampères. Fifty-six cells were used, each weighing 50 kilos. For a four hours' run each cell ought to have given out 110,000 kilogrammètres; it was, however, difficult to judge of the efficiency of these cells, as the charging dynamo was kept running all the evening, so that the accumulator acted more as a regulator than anything else. Seventy-six of these same cells were also used in the electrical launch.

Kornblüh of Vienna exhibited a great number of cells. He employs as framework of his plates a network formed of lead wire, into the interstices of which minium is pressed. The cell consists of ten plates, each 6 mm. thick, and weighing 30 kilos.

The secondary battery of J. J. Barrier, F. Tourville, and L. Godeau presents some novelties. It consists of four lead cylinders about 30 cm. high, placed one inside another, the diameter of the outermost being about 10 cm. In these cylinders are cut grooves about 1 mm. apart, which are filled in with a mixture of litharge and syrup or glycerine.

The cell of De Cato is constructed of six plates sewn into small bags, and kept apart by strips of wood. The plates consist of spongy lead treated with minium.

The general tendency in the new cells seems to be to make the plates thicker, at least 1 cm., and in some cases more. By this means greater durability is attained; for whilst a plate 0·5 cm. thick would last about three months, a plate 1 cm. thick may last five months or longer. Up to the present, however, accumulators seem chiefly to have been used as a means of regulating a current derived from a dynamo machine, and have not been much employed independently.

E. ZETZSCHE—TELEGRAPH APPARATUS AT THE VIENNA EXHIBITION.

(*Elektrotechnische Zeitschrift, B. IV., Nos.* 10 *and* 12, *October and December*, 1883, *pp.* 420 *and* 521.)

There was a very large and complete exhibit, both of historical and modern apparatus. In the English Section were shown portions of Ronald's

original telegraph, as well as the four- and five-needle instruments of Cooke and Wheatstone. Here were also to be seen a double-needle instrument of Cooke and Wheatstone of the year 1842, and the double-needle instrument used in the English Parliament in 1846, Nott's telegraph of 1844, and Bain's needle instrument. In the Austrian Section were exhibited Stohrer's needle telegraph of 1847, for induced currents, as well as a double-point writer with relay, by the same inventor, and the old form of Morse instrument with three writing points. Henley & Foster's telegraph, which was patented in 1848, and was used by the British and Irish Magnetic Telegraph Company, also found a place in the English Section.

In the Italian Section was a modification, by Hipps, of Bonelli's telegraph, and a specimen of Caselli's autographic instrument. This latter was also represented by the telegraphs of Von Meyer, D'Arlincourt, and Lenoir, in the French Section, in which were also exhibited the automatic instruments of Digney, and of Chauvassaignes, and Lambrigot; while in the English Section were Bain's three-key telegraph and his chemical telegraph. An interesting exhibit was the telegraph on the leakage principle, first used between Southampton and Cowes. In the French Section were shown a duplex Morse instrument with differential coils, as well as the combination of differential and bridge systems of duplex working used on the cables between France and Algiers. In the Austrian Section, Teufelhart exhibited his Hughes' duplex telegraph, with which experiments were first made between Vienna and Buda-Pesth; and O. Schaeffler exhibited two duplex apparatus, with an ordinary key and two relays for each, for open and closed circuits.

The author devotes a considerable space to the description of Baudot's and Meyer's multiple type-writers. The former is much used in France, as a sextuplex on the lines from Paris to Marseilles, and as quadruplex between Paris and Bordeaux, Lyons, Havre, and Lille respectively. Both these instruments have been fully described in English periodicals, and would not be clear without elaborate diagrams.

Two new forms of type-writers were exhibited in the Italian Section, one due to Faccioli, and the other to Dr. Alessandro Lucchesini, of Florence. The latter has taken special pains to ensure the absolute synchronism of the sender and receiver, which is of so vital importance in typé printing instruments.

Ed. Estienne has introduced an innovation into the Morse writer, by making the signs vertical in place of horizontal, the dots being half the height of the dashes, as shown in the example:  Postel-Vinay also showed an instrument on much the same principle as Estienne's, though the working parts were different. Both these instruments require a special form of sending key.

Amongst instruments giving a continuous trace may be mentioned Siemens and Halske's soot-writer and Thomson's siphon-recorder, as well as the instrument of S. Lauritzen for alternating currents used by the Great Northern Telegraph Company. Various forms of sounders were also exhibited, amongst others, Neale's acoustic dial.

VON HEFNER-ALTENECK—PHOTOMETRIC METHODS AND UNITS

(Elektrotechnische Zeitschrift, B. IV., H. 11, Nov., 1883, p. 445.)

The values obtained by measurements of electric lights, especially those produced by continuous currents, in a horizontal direction, cannot be relied on. According as the arc is on one side or the other of the carbons, the light emitted will vary enormously. It is generally recognised that the light should be measured in several planes, making greater or less angles with the horizontal. This may be most conveniently done by the aid of a mirror. A rod, which can be clamped on to the two vertical supporting rods of the lower carbon-holder of any ordinary form of arc lamp, carries at one end a metal screen, which prevents the direct rays from the arc impinging on the photometer; at the opposite end is a divided circle, about the centre of which turns an arm carrying a plane mirror. The apparatus is so fixed that the centre of the circle, and therefore the centre of rotation of the mirror, is just on a level with the arc. In every position the mirror is at equal distances from the arc, and it has such a slope that the angle included between the incident and reflected rays is always 90°. To determine the amount of light absorbed by the mirror, a comparison is made between the intensity of the light which is emitted directly by the arc and that reflected from the mirror. This determination once made need not be repeated, as the angle of reflection is always the same. From the measurements made, curves can be plotted for the varying angles—*e.g.*, with an upper carbon 11 mm. in diameter and a lower one 9 mm., and with a continuous current of 9·4 ampères and 45 volts difference of potential, the maximum light was found to be about 37° below the horizontal line; measured horizontally the light was equivalent to 370 candles, at an angle of 10° to 800 candles, and at 37° to about 2,000 candles. The question then arises, Which of these values shall be taken as giving the light power of the lamp?

In the above instance the light was unobscured by any globe, and if this case presents some difficulties, these are much increased when we come to consider lights enclosed in globes of more or less opaque glass. The use of a globe of some kind is necessary in order to diffuse the light more equally, and to do away with the sharp shadows which would otherwise be caused by the supports of the lamp and by the lower carbon. Globes of ground glass and of alabaster glass diminish the light about 15 per cent., opal glass about 20 per cent., and opaque, milk-white glass more than 30 per cent. This diminution of the light is not the same all round, but the brightest rays suffer most, the weaker ones not so much.

The effects of the several kinds have been studied by the author by means of a mirror such as has been already described, but of larger dimensions, so that the entire lamp and lantern could be suspended from a movable cross-beam, which was solidly attached to the mirror, so that both turned together. With the naked arc the maximum light of 1,976 candles was obtained at an angle of 35° below the horizontal line; with a globe of ground glass the maximum of 941 candles was at 30°; and with alabaster glass 652 candles at 35°. It is

most clearly shown by the curves of intensity for a continuous current that it is useless to put a reflector above the light, as, owing to the crater formed in the upper positive carbon, hardly any rays are emitted in a direction above the horizontal line. Owing to the uncertainty connected with the photometric values, particularly since the angle at which the measurement was made is rarely stated, it seems preferable to give the current in ampères which produces the light rather than the candle-power. The curves obtained for alternating currents, both with and without globes, were very nearly concentric circles, with the exception of course of the immediate upper and lower points.

In the author's opinion the best form of photometer is that of Bunsen, with a grease spot on a sheet of thin paper. For an easy comparison of two sources of light, it is, however, necessary that both sides of the paper screen should be visible at once without any movement on the part of the observer. Generally this has been obtained by placing two plane mirrors, making a somewhat obtuse angle with each other, behind the screen, which is thus perpendicular to their line of intersection. There is an objection to this arrangement, in that each mirror by projecting forwards throws its shadow on to the screen, so that the two illuminated images do not appear touching, but with a dark band between them. The author has introduced an arrangement which gets rid of this objection: instead of the mirrors behind the screen, he uses an obtuse-angled prism before the screen, between it and the observer's eye, and therefore out of the way of the rays falling on to the screen; so that the two illuminated images touch each other, and can be more exactly compared together.

All improvements of methods are, however, of little avail unless there be some well-defined unit of light in terms of which the light to be measured can be expressed. At the Congress of Electricians at Paris four units were considered—the Carcel burner, the standard candle, Violle's unit, and Draper's unit. Of these the first two only have been much used. Violle proposed as his unit the amount of light emitted by a square centimètre of surface of platinum kept just at the point of fusion. Draper's unit, which has been proposed also by Schwendler under a somewhat modified form, was the amount of light emitted by a platinum strip or wire of fixed dimensions maintained at incandescence by a constant electric current.

The standard candle, with its numerous drawbacks and deficiencies as a unit, is too well known to need any notice in the present abstract. There are four such standard candles in use—the English spermaceti candle, the stearine candle occasionally used in France, and called "*bougie de l'étoile*," the Munich stearine candle, and the German paraffin candle.

The Carcel lamp is of the moderator type, and has its oil reservoir in the base, from whence it is forced by a pump to the wick just below the flame, such oil as is not burnt running down again into the reservoir. Colza oil is used, and the consumption should be 40 grammes per hour: the burner is round and has an outside diameter of 23 mm.; but it is to be noted that the dimensions of this lamp as given in various descriptions do not agree.

It is not convenient to compare an electric light immediately with a

standard candle; and as the author has found that a petroleum oil lamp, when not giving its full light, burns very steadily, he prefers to estimate the intensity of such lamp by comparison with a standard candle, and then to compare the arc light with the lamp. With a view to find out how far a petroleum lamp can be depended upon, the author has made an extended series of experiments on various forms of lamp and with various kinds of petroleum; and with the consideration of the results obtained, which appear to him very satisfactory, he concludes his exhaustive article.

B. VIDOVICH—TWO NEW USES FOR THERMOPILES.

(*Centralblatt für Elektrotechnik, B. V., No.* 24, 1883, *p.* 529.)

Two wires, one of iron, the other of brass, each 1 mm. diameter and 10 cm. long, are twisted together through about 4 cm., the twisted portion is wrapped in muslin which is kept moist, and, according to the author, the apparatus can then be used, in connection with a sensitive galvanometer, as a hygrometer, since the current will depend on the temperature of the junction, and this latter on the rate of evaporation of the water from the muslin envelope, which depends again on the amount of moisture in the atmosphere. Three such couples were observed by the author during thirteen days, the deflections of the galvanometer being compared with the readings of one of August's psychrometers, with very promising results; but the apparatus requires elaboration.

The same arrangement might be used as an anemometer, as the author observed that the deflections of the galvanometer corresponded fairly well with the strength of an air current directed on to the moist muslin; the explanation of the phenomenon being, of course, that the stronger the current of air the more rapid was the evaporation and the lower the temperature of the junction.

H. DUBS—ELECTRICITY AT THE SWISS EXHIBITION.

(*Centralblatt für Elektrotechnik, B. V., No.* 25, 1883, *p.* 541.)

In a short letter the author notes some of the more important electrical exhibits.

Messrs. Bürgin & Alioth showed several dynamos, both for lighting and and transmission of power: in the latter the efficiency was 50 per cent. for a distance of one kilomètre. Transmission of power was also exhibited by the Société Genevoise pour la construction d'appareils de physique, with two Edison hundred-light machines.

A new machine by Thury was exhibited by Messrs. De Meuron & Cuénod, of Geneva. The armature of this machine very much resembles Edison's, and rotates between the poles of two horizontal electro-magnets, one on either side, in external appearance resembling the Gramme machine. Several sizes were shown, all shunt wound and for an E.M.F. of 110 volts, Edison lamps being used. All ran very steadily, and the one for 50 A lamps could be rotated

easily by passing through it the current of three bichromate cells. The smallest machine, for three lamps, weighed only 12 kilogrammes. A large machine for one arc light was also exhibited by the Zürich Telephone Company.

Dr. EMIL BOETTCHER—NEW SOLENOID GALVANOMETER.

(*Centralblatt für Elektrotechnik, B. V., No. 28, 1883, p. 620.*)

To a spring-balance (one of Salter's balances for weighing letters answers perfectly) is suspended a cylindrical core of soft iron 1 to 1·5 cm. thick and 20 cm. long, so that it hangs quite freely in the centre of a coil, fixed to the stand of the spring-balance, also 20 cm. long. The core hangs half its length below the bottom of the coil, so that when a current circulates in the latter the core is drawn upwards. The instrument is therefore an electro-magnetic balance, the downward attraction of gravity being opposed to the combined upward pull of the current in the coil and of the spiral spring. The instrument is very simple in construction, and can stand a good deal of rough usage. From the curves obtained by the author when using this instrument, it is apparent that for the same position of the core the upward attraction is proportional to the square of the current. The curves are in no case parallel to the axis of abscissæ—that is, the electro-magnetic attraction of cylindrical or double-coned cores by coils varies continuously with the position of the core: it reaches its maximum value when one-quarter of the length of the core projects beyond the coil. For practical use the author only graduates his instruments up to the point when three-quarters of the core are within the coil—*i.e.*, up to the point of maximum attraction. Within this limit a single observation suffices for graduating the scale, since the attraction is proportional to the square of the current, and also is increased or diminished by two per cent. for each centimètre of the rising or falling core. When not in use the weight of the core is borne by a cap fitting on to the bottom of the coil, with a bayonet joint.

H. HERTZ—BEHAVIOUR OF BENZINE AS AN INSULATOR AND DIELECTRIC.

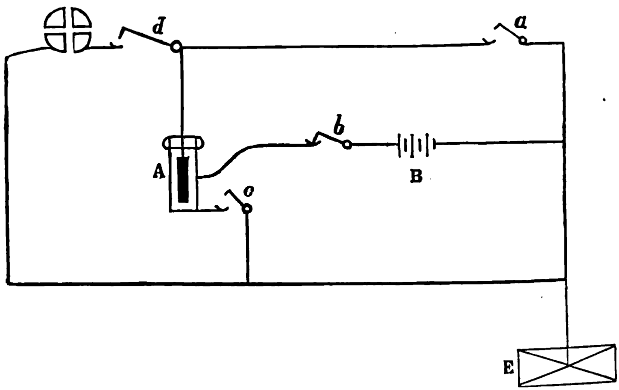
(*Annalen der Physik und Chemie, B. XX., H. 2, No. 10, 1883, p. 279.*)

The form of condenser used and the arrangement of the apparatus is shown in the accompanying figure, which scarcely needs explanation.

The benzine was contained in a cylindrical zinc vessel, A, and in it was suspended by two wires a zinc plate, the whole forming a Leyden jar. Four keys are shown. By depressing *a* the inner plate is put to earth, while the outer zinc could be put to earth by depressing *c*, or joined up to the battery by using *b*. If *a* and *b* are closed while *c* is open, a current, the strength of which will depend on the resistance of the benzine, will circulate. If now the key *a* is opened, the inner zinc plate will be inductively charged, and the needle of the electrometer will be deflected. The ratio of the capacity of the electrometer

to that of the benzine condenser was 4·5 to 1, the full potential of the battery deflected the electrometer needle through 5,500 scale divisions. Suppose now that one second after opening *a*, contact is broken at *d*, and that a deflection, *p*, resulted, then the difference of potential of the two coatings had diminished in a second by $\frac{a}{5000}$ of its value; without the electrometer this would have happened (4·5 + 1) times as quickly, or it would have fallen $\frac{a}{1000}$ in one second; or by its $e^{4\pi}$ part in $\frac{4\pi \times 1000}{a}$ second. By dividing this last time by the dielectrical constant of the benzine, we obtain the specific resistance in absolute electrostatic units.

In order to observe the residual charge, the outer coating was disconnected from the battery at *b*, and put to earth at *c*. One second after closing *c*, *a* was opened. The residual charge then deflected the electrometer more and more until a maximum was reached, after which the deflection fell off, owing to slight conduction through the benzine.



When experimenting with ordinary commercial benzine, the author found that at first the resistance was very small, while the residual charge was ten per cent. of the original charge. Testing again after about half an hour, the resistance had increased, while the residual charge had decreased, until after a lapse of twenty-four hours the residual charge was nil, and the resistance infinitely great.

The low resistance and considerable residual charge first observed are due to impurities in the benzine, and when these phenomena have disappeared they can be brought back by agitating the benzine mechanically in any way. A specimen of benzine of low resistance may be made a good insulator by distillation over calcium chloride. The electrical properties of benzine can also be partly changed by the continued passage of a current of electricity. An idea of the insulation of purified benzine may be formed from the author's statement that a Leyden jar with benzine as dielectric would only

fall to half charge in about two hours. The residual charge, which is so great in the case of impure benzine, seems to be due to polarisation, and not to a soaking in of free electricity, as may be shown experimentally by running off the benzine immediately a is opened at the moment the residual charge would begin to accumulate: there is then a deflection in the same direction as there would have been had the benzine been left in the Leyden jar, and in the opposite direction to that which would have occurred had the electricity soaked into the dielectric.

K. WAITZ—INFLUENCE OF GALVANIC POLARISATION ON FRICTION.

(*Annalen der Physik und Chemie, B. XX., H.* 2, *No.* 10, 1883, *p.* 285.)

It has been observed by Edison, Siemens, and Koch that friction between two substances is diminished when, by the passage of an electric current, polarisation is set up on the surface of one of them, and the author has devoted some time to a more complete study of this phenomenon than had been before attempted.

A small porous cell was filled with acidulated water and closed with a cork, through which passed a platinum plate into the liquid below. The porous cell was suspended by means of a brass wire attached to the platinum plate below and to another platinum wire above; to the brass wire were attached a magnet and a small mirror to admit of deflections being read in the ordinary way with a scale and telescope. In close proximity to the magnet was fixed a solenoid, by passing a current through which the whole suspended apparatus could be set in rotation without being touched. The suspended cell was immersed in a vessel holding also acidulated water, and against it was pressed a strip of glass covered with thin sheet platinum, the pressure being regulated by a micrometer screw. If, now, the suspension wire and a second platinum wire dipping into the outer vessel were joined up to two Daniell's cells, so that a current passed through the whole apparatus, the friction between the porous cell and the platinum was greatly diminished; but if only one cell was used no alteration was observed in the amount of friction, the one cell being unable to effect the electrolysis of the acidulated water. The author, thinking that perhaps the passage of the current through the porous cell might have something to do with with the effect produced, then introduced a second electrode into the outer vessel, so that the current passed from this second electrode to the platinum in contact with the cell, but not through this latter; the diminution in the friction was, however, still as fully evident as in the former arrangement.

The surface of the porous cell was, however, found to be rather too rough; and in the long series of experiments made with platinum, palladium, gold, and nickel, in various different liquids, the clay cell was replaced by a glass cylinder, the mode of suspension being unaltered.

Observations were made of the changes in the friction between the glass and metal surfaces with varying E.M.F., this being changed at will by joining the lead to various points of a set of resistances which were connected as a

shunt across two Daniell cells. The twists given to the glass cylinder by the attraction of the suspended magnet by the solenoid were made to follow each other at regular intervals, the circuit through the solenoid being closed and opened by the action of a swinging pendulum.

The experiments may be briefly recapitulated, it being remembered that the friction occurred in each case between the metal mentioned and glass :—

1. *Platinum in Dilute Sulphuric Acid.*—There was a decrease of friction with increase of H polarisation, and an increase with increase of O polarisation.

2. *Platinum in Solution of Soda.*—The same decrease in the friction occurred with increase of H polarisation; but the increase of the friction by O polarisation was not nearly so considerable as in the case of sulphuric acid.

3. *Platinum in Solution of Potash.*—No particular alteration in the friction was observed.

4. *Platinum in Ferrocyanide of Potassium.*—H polarisation caused an increase in the friction at first, till an E.M.F. of about half a Daniell was reached; it then fell off again, and with one Daniell E.M.F. had reached its original value. O polarisation seemed sometimes slightly to diminish the friction, but more generally it had no effect.

5. Palladium in dilute sulphuric acid had the same effect as platinum. (*See* 1.)

6. Palladium in solution of soda had very little, if any, effect on the friction.

7. *Palladium in Solution of Potash.*—H polarisation considerably diminished the friction, while O polarisation slightly increased it.

8. *Gold in Dilute Sulphuric Acid.*—Koch, in his experiments, found no effect; but the author proved that an increase of friction took place with O polarisation, and a decrease with H polarisation.

9. *Gold in Solution of Soda.*—The effect was equally well demonstrated.

10. *Gold in Solution of Potash.*—A great decrease of the friction with H polarisation, and an initial increase with O polarisation, followed by a decrease.

11. *Nickel in Solution of Soda.*—A decrease in the friction with H polarisation. No results were obtained with nickel in other liquids.

As a generalisation, it may therefore be said that platinum, palladium, gold, and nickel, in liquids which do not act chemically on them, and which would evolve gases if decomposed, cause an increase in the friction of the metal with glass when polarised with oxygen, and a decrease when polarised with hydrogen, if the E.M.F. is not sufficiently great to decompose the liquid.

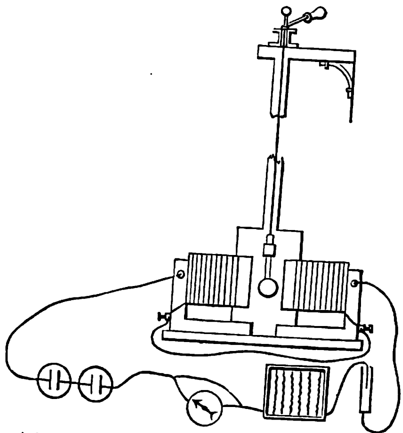
Though the experiments do not entirely clear up the cause of the phenomenon observed, still perhaps an explanation may be found in the formation of two electric layers, the one produced by the contact between the glass and the liquid, the other being formed on the polarised metal electrode. Since in the capillary space between the glass and the metal which is filled with the liquid, with a H polarisation parts of the two double layers of like sign occur, while with O polarisation these parts are of unlike sign, it might be thought that the different arrangement of the molecules in the capillary space in the two cases might modify the internal friction of the liquid in the way observed in these experiments.

F. STENGER—BEHAVIOUR OF CALCITE IN A UNIFORM MAGNETIC FIELD.

(*Annalen der Physik und Chemie, B. XX., H. 2, No.* 10, 1883, *p.* 304.)

The observations of Plücker, Faraday, Knoblauch, and Tyndall all lead to the conclusion that crystals, which do not belong to the regular system, become unequally induced, according to their position with respect to the lines of force of the magnetic field; that they therefore must take up a fixed position in a uniform field; but that they suffer attraction or repulsion in a field which is not uniform. All theories which have been proposed to account for the behaviour of crystalline media in uniform fields may be redued to one, viz., that a sphere of any crystal will show the same phenomena as a certain ellipsoid of an isotropic medium with three axes.

The substance chosen by the author was calc-spar, as this mineral possesses magnetic action in a marked degree, and, in order to get rid as far as possible of the influence of the form of the crystal, this was always used in shape of a sphere. The field of force was produced by an electro-magnet of Ruhmkorff's shape, with flat pole-pieces 17 cm. in diameter, and 8·8 cm. from each other. The calcite ball, about 4·4 cm. in diameter, was hung exactly between them by a bifilar suspension of silver wire three inches long. The general arrangement of the apparatus is apparent from the accompanying sketch.



The crystal ball itself was at first held between two horizontal glass plates kept together by three threads, the upper plate being fixed to a brass rod, which, in its turn, was fixed to the bifilar suspension. As the result of experiment, it was found, however, that this arrangement had a disturbing influence on the observations, and finally the brass rod was replaced by a wooden one, to which the ball was stuck with a little wax. The constancy of the current which excited the electro-magnet was maintained by altering the

amount of resistance in circuit, the intensity being read on a tangent galvanometer in a shunt circuit. The time of oscillation of the suspended system was determined by means of a small mirror and scale. In order to get rid of every trace of torsion in the silver suspension wire, it was hung up with a stretching weight at the lower end, and then heated by the passage of a current, the connection above being made directly, while below the wire dipped into a mercury cup.

The optical axis of the ball of calcite was very accurately determined in a polariscope, and the position marked on the ball by two spots of Indian ink. The experiments were made with the ball fixed in all sorts of positions, so that the angle included between the axis of rotation and the optical axis varied from 0° to 90°. The ball having been placed in the apparatus, the torsion head at the top was turned until no movement took place on closing the circuit of the electro-magnets, thus showing that the optical axis was in the equatorial plane of the magnet. The time of oscillation of the ball was then observed when the magnet was excited. and the angle measured which the optical axis made with the axis of rotation.

The author develops the equation, expressing Thomson's theory of induction, and, comparing it with his experiments, concludes that it does not explain the behaviour of a crystal in a magnetic field, since it does not take into account the coercive force of the body nor the induction of the several particles on each other. So far as the author's experiments have yet led him, he is not able to put forward a satisfactory theory; but new and improved methods of research will be tried by him, in order to arrive at a satisfactory result.

For comparison of the results already gained with these future experiments, it was desirable to determine the intensity of the field of the electro-magnet in absolute units. A small coil of fine wire, 14 mm. outside diameter and 6·9 mm. inside diameter, was supported on a stand at various points of the field, and could be rotated through 90° from a position where its axis was parallel to the lines of force, to another where the two were at right angles. The coil was connected up to a Gaugain's galvanometer. We then have the equation

$$F = \frac{C\,t\,a\sqrt{k}\,w}{\pi f}$$

where F = intensity of the field, C = factor of the galvanometer, t = time of oscillation of galvanometer needle, a = deflection in radians, k = coefficient of damping of needle, w = resistance of whole circuit, and f = effective area of the coil. On substituting the several numerical values, the author found

$$F = 58{\cdot}098\,\frac{g^{\frac{1}{2}}}{\text{cm.}^{\frac{1}{2}}\,\text{sec.}}$$

J. BORGMANN—PHOTO-ELECTRIC BATTERY.

(*Beiblätter, B. VII., St.* 9, 1883, *p.* 715. *Jour. d. Russ. Phys.-Chem., Ges.* 14, *p.* 258, 1883.)

In order to demonstrate the production of a current by the action of light, the author constructed a battery of several U-shaped glass tubes, which were filled with dilute sulphuric acid, and into the legs of which dipped small plates of silver iodide. The several tubes were joined up in series. Diffused daylight, when allowed to fall on to the one limb of each tube, produced sufficient current to cause a considerable deflection of the needle of a Wiedemann's galvanometer. Magnesium light is a better illuminant. Such cells remain sensitive to the light for a long time.

IDEM—HEATING OF IRON BY ALTERNATE MAGNETISATION.

(*Beiblätter, B. VII., St.* 9, *p.* 721, 1883. *Jour. d. Russ. Phys.-Chem., Ges.* 14, *p.* 67, 1883.)

The numerous observations which have been made on this subject have not set at rest the question, whether the heating is due to the magnetisation directly, or to the development of induced currents in the mass of the iron. The author has attempted a solution of the question by experimenting on tubes slit longitudinally, of identical dimensions and under identical circumstances, the one set of tubes being of iron and the other set of copper. Two glass tubes, 500 cm. long and 4·5 cm. wide, formed the reservoirs of two air thermometers: in the one tube were placed the iron tubes and in the other the copper ones. The rise of temperature was measured by a manometer filled with naphtha. These glass tubes were surrounded by other larger tubes, water at the temperature of the room circulating between the two. Each tube was placed in a solenoid of 2 mm. copper wire with 540 convolutions, which was surrounded in turn by an induction coil of thin wire with 1,150 convolutions. Any one of the coils could be connected by a commutator with a Weber's electro-dynamometer. Both the magnetising solenoids of thick wire were in circuit with a battery of 4 to 10 cells of Poggendorf; the current, by means of a commutator such as is used in the Gramme machine, and which was driven by clockwork, could be interrupted five, ten, or twenty times per second, or could be reversed in direction six or twenty times per second. By observations of the deflections of the electro-dynamometer under the action of either of the induction coils, it is possible to deduce relative values for the magnetic moment of the iron tubes experimented upon, and thus to compare the quantity of heat given off with the magnetism induced in the tubes.

The results of a great number of observations showed no heating of the copper tubes, which must have occurred if the observed heating of the iron was only due to the development of induced currents. With the iron tubes it was found that with interrupted magnetisation more heat was produced than when the magnetisation was reversed. In the case of iron which had been previously submitted to reversals of magnetism, the heat given off varied proportionally with the number of interruptions per second, and

increased very nearly in proportion to the square of the temporary magnetism. For iron tubes of the same thickness and dimensions, it was found that those not slit longitudinally gave off one and a half times more heat than such as were slit. Tubes of antimony also showed traces of a rise of temperature when alternately magnetised and demagnetised. The author concludes that the observed heating, when iron is alternately magnetised and demagnetised, is a result of the joint action of the vortex movements of the æther and of the material of the body.

G. VICENTINI—MODIFICATION OF THE COILS OF ELECTRO-MAGNETS.

(Beiblätter, B. VII., St. 9, p. 720, 1883. Ann. del R. Ist. Tecnico di Torino, 9, p. 1, 1882, Sep.)

The ordinary coils are replaced by a thin sheet of copper, the width of which is equal to the height of the legs of the magnet; the successive layers of the copper sheet are insulated with shellac and a silk ribbon. The maximum magnetic moment is obtained if the number of turns is increased until the resistance of the strip has the same ratio to the external resistance as the thickness of the bare strip to that of the insulated strip. To arrive at the best results, Müller's rule should be followed, viz., that the diameter of the core should be equal to the thickness of the magnetising coil, in which case the resistance of the latter is twice the external resistance, and the length of each leg of the magnet should be six times its diameter.

Dr. A. R. HARLACHER—ELECTRIC METER FOR MEASURING THE VELOCITY OF RIVERS.

(Internat. Zeitschrift für die Elek. Ausstell. in Wien, 1883, No. 12, p. 189.)

To a vertical rod which can be lowered into the river is attached a horizontal bar, movable by means of a sleeve round the rod. To one end of the horizontal bar is fixed a vane, which, by the action of the water, always points down stream; the other half of the rod, pointing up stream, carries a two-bladed screw, the spindle of which turns in agate cups at either end, and is hence insulated from the body of the instrument. On the spindle is fixed a collar, made half of platinum and half of vulcanite, against which presses a light platinum spring in connection with a binding screw (A). The spindle also carries an endless screw gearing into a wheel with fifty teeth, which at one point has a platinum pin projecting laterally. Above the toothed wheel and rather to the side, is a light spring in connection with a binding screw (B); this spring touches the platinum pin on the wheel once in each revolution, *i.e.*, once for every fifty turns of the screw spindle. The line used for lowering and supporting the instrument contains two insulated wires leading up to the recording apparatus. This is twofold, one part consisting of a single-stroke bell, the other of an electrical counter. The counter has an electro-magnet

inside which attracts the armature on the passage of a current, and the movement of the armature advances the index of the counter through one division.

There are two ways of employing the apparatus. If the leading wire is connected to terminal (B) and to the bell, then one stroke will be given every time the platinum pin on the toothed wheel makes contact with its spring, *i.e.*, the completion of every fifty turns of the screw will be marked by a stroke on the bell. It is then easy with the help of a good chronograph to find the number of revolutions per second, and hence the velocity of the stream. If the leading wire be connected to screw (A), then every time the platinum half of the collar on the screw spindle comes in contact with its spring, the circuit is completed through the counter which is used in this case, and the revolutions are recorded one by one on the dial. The electrical counter and the bell, together with a small drum for coiling up the wire and the battery, are contained in a portable case. In place of the counter a Morse writer could be employed, and if one with two writing wheels is used, and a suitable addition made to the immersed portion of the apparatus, it is possible to obtain a record, not only of the number of revolutions of the screw, but also of the varying depths to which the apparatus is lowered. The description gives details of some of the mechanical parts of the apparatus which are not of immediate interest.

GISBERT KAPP—BEST RATIO OF IRON AND COPPER IN THE GRAMME RING.

(*Internat. Zeitschrift für die Elek. Ausstell. in Wien, No.* 15, 1883, *p.* 232.)

From direct experiment the author has arrived at the conclusion that the E.M.F. of a dynamo machine may be very approximately expressed by the product of five factors, viz., the number of lines of force which pass through the unit area of the ring (μ), the number of commutator plates (N), the area of the iron ring in square inches (a), the number of convolutions corresponding to each commutator plate (t), and number of revolutions per minute (n); or in symbols, $E = \mu\, N\, a\, t\, n$. μ is a coefficient which is determined empirically for each type of machine.

By the aid of such a formula we are able to solve the following problem:—Given the size of the machine and the strength of the normal current,—that is, given the size of wire which must be used to avoid heating by the current, and the space which can be occupied by the armature,—to find the proportion of iron to copper which shall give a maximum E.M.F.

Suppose $a\ b\ c\ d$ the space available for the copper and iron in one half of the area of a Gramme ring, the length of which is L and the thickness H. Let the corresponding dimensions of the iron core be l and h, and s the thickness of the layer of copper wire wound on:

$$l = L - 2\,s\,;\ h = H - 2\,s.$$

The number of convolutions can with sufficient accuracy be taken as proportional to the thickness, s, of the layer, so that the E.M.F. is proportional

to the product of three factors, h, l, and s. We have now to find the value for which

$$h\,l\,s = s\,(L - 2\,s)\,(H - 2\,s)$$

is a maximum. Differentiating and equating to zero, we have

$$H\,L - 3\,s\,(H + L) + 8\,s^2 = 0,$$

which will be a maximum when

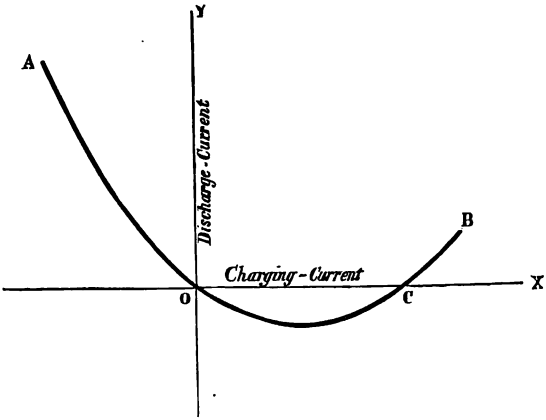
$$s = \tfrac{3}{16}\,(H + L) - \sqrt{\left\{\tfrac{3}{16}\,(H + L)\right\}^2 - \tfrac{1}{8}\,H\,L.}$$

For $L = 4$ to $8\,H$, s is approximately $\tfrac{1}{5}$ H, a value generally found in practice in Gramme machines.

Dr. A. VON WALTENHOFEN—EXPERIMENT WITH THE THERMOPILE OF NOË-REBICEK.

(*Zeitschrift des Elektrotech. Vereines in Wien, B. I., H.* 7, 15*th Oct.*, 1883, *p.* 225.)

If a current from a source of electricity is passed for a few minutes through a thermopile, on interrupting what may be called the charging

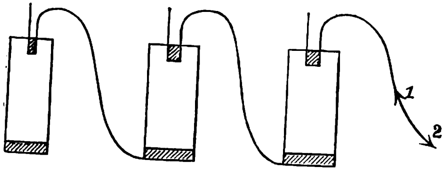


current, and connecting the thermopile to a galvanometer, a discharge current will be observed, which may be called the Peltier current. If a series of observations are made with a Noë thermopile, with a constantly increasing strength of the charging current, it will be remarked that, when the charging current passes at the hot junctions from the positive to the negative metal, which direction of the current may be called the negative one, the discharge current increases with the charging current, and is in the opposite direction. If the charging current is positive,—*i.e.*, passes from the negative to the positive metal,—the discharge current, which is at first in the opposite direction, increases with increase in the charging current, then decreases with increase in the latter, passes through a zero point, and, on further increasing the charging current, finally flows in the same direction and again increases.

The above curve illustrates the results obtained.

For positive charging currents, the curve O B, which is concave to the X axis, represents the strength of the discharge current, while the curve O A, convex to the X axis, represents the discharge current when the charging current was in the negative direction.

The following diagram may help to make clear what the author means by positive and negative charging currents, three elements of the Noë thermopile being shown:—



The arrow-head 1 shows the direction of the positive current, the arrow 2, the negative.

The strength of the charging current varied from 1·6 to 7·7 ampères. The passage of the discharge current through the zero value corresponded to a charging current between 6 and 7 ampères. The maximum of the negative discharge current corresponded to a charging current of 3 ampères, and the E.M.F. was about 0·04 volt. If the charging current is denoted by s, and the discharge current by σ, then $\sigma = \pm a s + b s^2$, where a and b are constants.

The above-described phenomena do not occur with all kinds of thermopiles. They appear to be due to the fact that the resistances of successive junctions in the cell are not equal. Thus, in the Noë pattern, the resistance at that point of each element where the German silver is soldered to the heated point is greater than the resistance at the point where the wire is soldered to the wide end of the next cell. (See sketch.)

H. VON JUEPTNER—EFFECT OF MAGNETISM ON THE ELECTROLYTIC PROPERTIES OF THE METALS.

(*Zeitschrift des Elecktrotech. Vereines in Wien, B. I., H.* 8, 1*st Nov.*, 1883, *p.* 244.)

The deposition of a metal from a solution of one of its salts is accompanied by electrolytic action, and this action can be modified by the influence of other forces, as has been demonstrated by Ira Remsen. He filled a flat vessel of sheet iron with a solution of copper sulphate, and placed it on the poles of a permanent horse-shoe magnet of Jamin's construction. On pouring off the liquid, after one or two minutes, he found the bottom of the vessel covered with copper, with the exception of certain lines which reproduced the shape of the magnet poles underneath. These lines appeared as depressions in the surface of the deposited metal, showing that at these points the intensity of the reaction had been diminished. Other lines not quite so decided could be traced, which followed the equipotential lines of the magnetic field. The experiment was then repeated with only one magnet hole, when, on pouring off the solution of copper sulphate, it was found that no copper had been deposited

immediately over the pole, while the thickness of the deposit increased regularly with the distance from the pole, the lines in this case being concentric.

The author sees the reason for the result obtained, in the fact that the action of the magnet on the particles of iron of the vessel was opposed to the action of the solution which tended to dissolve the iron and to precipitate copper. If a solution of a salt of iron were placed in a zinc trough under similar circumstances, the reverse action should take place, and the iron should be deposited most thickly over the poles of the magnet.

A. CHERVET—NEW CAPILLARY ELECTROMETER.

(*Comptes Rendus, T.* 97, 17*th Sep.*, 1883, *p.* 669.)

Two glass flasks, A and B, are connected by a thermometer tube passing through lateral tubulures. A is filled with mercury, B with mercury and dilute sulphuric acid (1 in 10); the reservoir end of the thermometer tube is fixed in A, the capillary end opens into the water in B. A platinum wire, P, is immersed in the mercury in A, and a second wire, N, passes through a glass tube into the mercury in B, so as not to be in contact with the acidulated water. The wire P should always be positive. The height of the mercury and water in the two bottles is such that when P and N are joined up, the surface of separation of the two liquids is just at the level where the enlarged part of the thermometer tube joins on to the capillary part.

With such an instrument it is possible to perceive a difference of potential of one ten-thousandth of a volt. Suppose M a quantity which varies with the difference of potential between P and N, then $M = a\, r$, where $a =$ the capillary depression, and $r =$ the radius of the tube. If the difference of potential increases, M increases, hence r increases, *i.e.*, the meniscus moves towards the bottle A. If V (the difference of potential) $= 0{\cdot}001$ volts, Lippmann has shown that M increases by $\frac{1}{750}$ of its value. Let x be the corresponding displacement, the radius of the tube becomes $r + x$, sin. α, where α is the angle of the cone which is tangent to the surface of the tube at the point where is the meniscus.

$$\therefore a = \frac{M}{r} = \frac{M + \frac{1}{750}M}{r + x \sin.\alpha} = \frac{\frac{1}{750}M}{x \sin.\alpha}$$

or

$$x \sin.\alpha = \frac{r}{750}$$

If $a = 1$ cm., $r = 0{\cdot}45$ mm., x must be greater than $0{\cdot}2$ mm. to be visible to the eye; hence

$$\sin.\alpha = \frac{0{\cdot}45}{750 \times 0{\cdot}2}, \text{ and } \alpha = 10 \text{ minutes of arc about.}$$

We can thus read directly displacements corresponding to $V = 0{\cdot}001$, and, by employing a lens magnifying ten times, up to $V = 0{\cdot}0001$.

The measurements are made in the following way:—Let V be the differ-

ence of potential between P and N, the meniscus is displaced towards A; now bring it back to zero by exerting a pressure, p, on the mercury in A. This pressure can be most conveniently determined by a water manometer, or, better, by the differential manometer of Kretz with dilute alcohol and turpentine.

To show that this very small pressure, p, can be accurately measured, suppose $V = 0{\cdot}0001$ volt, according to Lippmann the pressure necessary to bring the meniscus back to zero will be $\frac{1}{7500}$ of the capillary depression, or, on Kretz's manometer, 391·5 times greater; hence, the capillary depression being 1 cm.,

$$p = \frac{1}{7500} \times 391{\cdot}5 = 0{\cdot}5 \text{ mm.},$$

a length which can easily be measured.

To calibrate the instrument, it is only necessary to determine the pressure, p, corresponding to $V = 1$ volt.

E. BUDDE—MERCURY INTERRUPTER WORKING IN HYDROGEN GAS.

(*La Lumière Electrique, V. X., No. 37, 15th September, 1883, p. 94.*)

When a mercury contact-breaker works in air, it is well known that the surface of the mercury rapidly oxidises under the influence of the sparks produced on breaking contact. In order to get rid of this inconvenience, the author surrounds the contact-breaker with an atmosphere of hydrogen. He uses a form of contact-breaker very similar to that of Foucault, and he covers it with a bell jar. A current of pure dry hydrogen is passed into this bell jar, having first passed over strong sulphuric acid and calcium iodide. When the air in the bell jar has been displaced by hydrogen, the contact breaker is set to work, and the current of hydrogen is stopped, almost completely. The author states that under these conditions the surface of the mercury remains quite bright after four hours' use.

B. F. THOMAS—METHOD OF CALIBRATING GALVANOMETERS.

(*La Lumière Electrique, V. X., No. 42, 20th October, 1883, p. 252.*)

A battery is connected up in circuit with the galvanometer to be calibrated, a galvanoscope, a box of resistance coils, and a key. The resistance is adjusted so that the galvanometer shows its maximum deflection; the needle of the galvanoscope will then be hard over against its stop. Bring back the galvanoscope needle to zero by means of a permanent magnet. Now shunt the galvanometer by a resistance equal to its own resistance, and adjust the resistance in circuit until the galvanoscope again comes to zero. The current flowing in the circuit will then be the same as before, while obviously the current through the galvanometer is half its original value. By shunting the galvanometer with resistances which are multiples and submultiples of its own resistance, we shall obtain various currents which will be multiples and submultiples of the original current. From the values of these currents and from the deflections a curve can then be plotted.

E. REYNIER—EXPERIMENTS ON LOCAL ACTION OF ZINCS ON OPEN CIRCUIT.

(*L'Électricien, V. VI., No.* 60, *October* 1, 1883, *p.* 296.)

In order to diminish as far as possible any local action, the methods usually adopted are the use of very pure zinc, which is difficult to obtain, and amalgamation, which is troublesome and costly. The author has proposed to place the zincs in separate compartments, which is fairly efficacious, but adds to the internal resistance. The use of liquid amalgams of zinc is again too costly. Leclanché tried solid amalgams, but we have no data of the results obtained. Since then M. Dronier has reproduced these solid amalgams; but no thorough investigation of the subject having been made, the author was led to undertake a series of experiments.

In all the experiments the zincs were cylindrical, of the well-known form employed in the Leclanché cell. The dimensions were 165 mm. long by 10 mm. in diameter, and the weight 90 grammes. The bare zincs were drawn like wires. The amalgamation was effected with pure mercury, after washing in solution of caustic potash and in dilute sulphuric acid. The amalgamation adds about half a gramme to the weight, but an unknown quantity of zinc is dissolved.

The solid amalgams contained four per cent. of mercury. They were prepared by throwing the quantity of mercury required into molten zinc. Each liquid was tested with the three kinds of zincs mentioned above. About a litre of liquid was used in each case, and the surface immersed was from 31 to 38 square centimètres. The author has tabulated his results, of which the following are a selection; the figures in each case are the weight in milligrammes lost per square centimètre per hour:—

Liquid.	Bare zinc.	Amalgamated.	Solid. amalgam.
Dilute sulphuric acid 10 per cent.	2,965	54	7
Dilute sulphuric acid 10 per cent. and nitrate of soda	3,794	12	8
Dilute sulphuric acid 10 per cent. and sulphate of copper ...	4,698	4,244	2,393
Sulphate of copper	117	44	57
Bichromate solution	7,810	1,991	1,334

From a consideration of the results the author is led to the following conclusions:—The protection afforded by amalgamation is much greater than is usually supposed. The solid amalgam shows itself very superior, especially in experiments of long duration. In the ordinary amalgamated zincs it is the outside layers which contain the most mercury, and the eating away therefore proceeds the more rapidly the deeper the action goes, while the reverse is the use with the solid amalgam. Fuming sulphuric acid is to be preferred to the ordinary kind. The presence of free nitric acid increases the local action. The addition of a little sulphate of ammonium in a solution of bisulphate of potassium diminishes greatly the action, while sulphate of copper in dilute sulphuric acid increases it enormously. In the bichromate solution the solid

amalgam is the only one which resists the action to any considerable degree. The final conclusion is that the solid amalgam is far superior to the ordinary amalgamated zincs or to bare zincs, while its original higher price is counterbalanced by the fact that it does not require re-amalgamation.

L. LOSSIER.—SEPARATION OF METALS BY ELECTROLYSIS.

(*L'Électricien, V. VI., No.* 60, *October* 1, 1883, p. 304.)

The basis of modern electro-chemistry is the principle put forward by Sir Wm. Thomson, that in order to decompose a chemical compound by an electric current, it is necessary that the energy furnished by the current should be greater than that which has been liberated by the formation of the compound.

According to the author, this law is not strictly exact. Thus, if a current is passed between two silver electrodes dipping into a solution of sulphate of copper, a deposit of copper is formed on the negative electrode, the decomposition of the sulphate using up 29·4 calories. At the positive electrode sulphate of silver will be formed, giving rise to 10·7. Hence, on the above theory, the difference, 18·7 calories, which correspond to 0·82 volt, will have to be furnished by the current. But the author has repeatedly found that electrolysis will take place in this case with less than 0·82 volt, indeed with any E.M.F. If the current is very feeble, it may be some days before the appearance of a cloud about the positive electrode shows the formation of sulphate of silver. The action is at first very limited, but after a time the sulphate of silver becomes diffused through the liquid, and, reaching the negative electrode, is decomposed.

The author is led by his experiments to the conclusion that it is not practically possible to separate two metals from an alloy by electrolysis. Thus, in the case of an alloy of copper and silver, according to the theory quoted, if the E.M.F. is kept below 0·82 volt, say, at 0·5 volt, no silver should be precipitated, but the author, by a large number of experiments, has verified the fact that the silver also is dissolved.

In order to effect the separation of two metals, it is necessary to proceed somewhat differently. When the cathode in a solution of sulphate of copper is a copper plate, and the anode a silver plate, the couple form a battery, with a difference of potential of 0·29 volt at the silver electrode. If now the positive pole of a battery of 0·29 volt E.M.F. be connected to the silver plate, and the negative pole to the copper, the current will be nil. In order to separate an alloy of three parts copper and one part silver, the plates should be arranged as above, taking care that the fall of potential from the anode to the cathode is always less than 0·29 volt; the copper alone will then be dissolved, leaving the silver as a porous mass. The author has applied the same method to the separation of zinc and copper, gold and silver, etc., and even of nickel from cobalt and iron.

LIST OF OTHER ARTICLES.

(*Elektrotechnische Zeitschrift, B. IV.*)

H. 10.—**J. LUDEWIG**—Earth-current Observations. **Dr. ARON**—Telephones at Vienna Exhibition.

H. 11.—**J. LUDEWIG**—Earth-current Observations. **H. DISCHER**—Calculation of Artificial Resistances for Duplex Working on the Bridge Principle.

(*Centralblatt für Elektrotechnik, B. V.*)

No. 25.—Recent Advances in Incandescence Lighting.

No. 26.—Theoretical Glow of the Magnetic Field—Motors at the Vienna Exhibition.

No. 27.—Electrometer of Mascart.

No. 28.—Dynamos and Lamps at the Swiss Exhibition—Electrometer of Edelmann.

(*Beiblätter, Bd. 7, St. 9.*)

G. R. DAHLANDER—Potential and Capacity of a System of several Conductors.

A. RIGHI—Electric Shadows.

(*Dingler, Bd. 249.*)

H. 10.—**L. ZEHNDER**—Atmospheric Electricity.

H. 11.—Cable with Non-inflammable Serving.

H. 12.—**UPPENBORN**—Measurement of Work done in Conductors.

(*Exner's Repertorium der Physik, Bd. 19.*)

H. 8.—**MACH**—Experiments with Melsen's Lightning Conductors.

H. 9.—**A. KURZ**—Magnetic and Torsion Pendulum. **VON SCHAIK**—Electro-magnetic Rotation of Plane of Polarisation of Light.

(*Internationale Zeitschrift für die Elektrische Ausstellung in Wien.*)

No. 9.—**ZENGER**—Universal Rheometer. **L. KRÄMER**—Railway Telegraphs and Signals.

No. 10.—**KOHLRAUSCH**—Popular Exposition of Chief Laws of Electro-Dynamics.

No. 11.—Ditto—Ditto.

No. 12.—**A. WILKE**—Heating by Electricity. **KOLACEK**—Theory of Gramme Ring.

No. 13—**ZENGER**—Construction of Lightning Rods. **L. KRÄMER**—Railway Telegraphs and Signals. **LEWANDOWSKI**—First Use of Electricity in Medicine. **ROSS**—Electrical Cooking Apparatus.

No. 14.—**KOHLRAUSCH**—Difference between Dynamo and Magneto Machines.

No. 15.—**WALLENTIN**—Planté's Accumulator. **VOLKMER**—Electrical Chronographs for Projectiles.

No. 16.—**L. KRÄMER**—Railway Telegraphs and Signals.

No. 17.—**HANDL**—Batteries.

(*Zeitschrift des Elektrotechnischen Vereines in Wien, B. I.*)

No. 3-4.—**Dr. WALTENHOFEN**—Efficiency of Electro-magnetic Motors. **H. DISCHER**—Schwendler's Duplex System. **Prof. DIETRICH**—Measurements of Dynamos. **E. FERRARIS**—Dynamos for Electrolysis. **L. PFAUNDLER**—Comparison between Dynamos of Kravogl and Gramme.

No. 5.—**E. FERRARIS**—Dynamos for Electrolysis. **L. PFAUNDLER**—Comparison between Dynamos of Kravogl and Gramme.

No. 6.—**WASSMUTH**—Theory of Dynamos. **E. FERRARIS**—Dynamos for Electrolysis.

No. 7.—**DVORAK**—Theory of Flame Discharge. **GRANFELD**—Earth Currents.

No. 8.—**REINISCH**—New Proof of Joule's Law.

(*Comptes Rendus, Vol. 97.*)

No. 9.—**CABANELLAS**—Measurement of Difference of Potential and of Resistance between Electrodes.

No. 11.—**QUET**—Laws of Induction.

No. 12.—**CABANELLAS**—Electrical Law of the Conservation of Energy.

No. 13.—**QUET**—Laws of Induction.

No. 15.—**QUET**—Laws of Induction.

(*La Lumière Electrique, Vol. 10.*)

No. 39.—**MOUTIER**—Theory of Electrostatic Phenomena.

No. 40.—**MOUTIER**—Theory of Electrostatic Phenomena. **MINET**—Galvanometric Measurement of E.M.F. of a Battery.

No. 41.—**E. FERRARIS**—Shunt Dynamos. **BECQUEREL**—Rotation of Plane of Polarisation by Terrestrial Magnetism.

No. 42.—**MOUTIER**—Theory of Electrostatic Phenomena.

No. 43.—**MOUTIER**—Theory of Electrostatic Phenomena. **MINET**—Commercial Efficiency of Incandescence Lamps.

(*L'Electricien, Vol. 6.*)

No. 57.—**VIOLLE**—Photometry of Arc Lights. **JAMINE**—Polarisation of Accumulators.

No. 62.—**HOSPITALIER**—Faure-Sellon-Volkmar Accumulators in Practical Use.

JOURNAL

OF THE

SOCIETY OF
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The One Hundred and Thirty-first Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 28th February, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and approved, and the names of new candidates were announced.

Donations to the Library were announced as having been received from the editor of "The Electrician," Lord Rayleigh, the Cornell University, Mr. W. M. Mordey (Associate), and Mr. J. Aylmer (Local Honorary Secretary for France), and the meeting accorded its thanks for the donations.

The PRESIDENT: I regret to have to announce to the members of the Society the death, on the 18th of February, of one of our Foreign Members, the Count Th. du Moncel, membre de l'Académie des Sciences, who was well known wherever electricity is studied or applied. He was a clear writer on electricity and its applications, and, as editor of "La Lumière Électrique," was very well known to all workers in the electric world.

Now that we are legally constituted and registered as an incorporated society, we are required by our articles of association to submit an annual statement of accounts to the general meeting. The statement for the year 1883, as audited, has been

distributed to members this evening, and the Honorary Treasurer and Secretary are prepared to answer any questions which members may desire to put on points which may require further explanation. If the statement submitted for your approval is satisfactory, and if no one desires to put any question, I would propose that the annual accounts for the year 1883, as now submitted, be received and adopted.

The proposal, having been seconded by Mr. James Mackenzie, Member, was duly carried.

The PRESIDENT announced that a mishap had prevented Professor Forbes from reading the paper which had been announced by him, but that gentlemen would substitute two other brief papers in place of it.

The following paper was then read:—

ON SOME PREJUDICIAL ACTIONS IN DYNAMO MACHINES.

By W. M. MORDEY.

In converting mechanical energy into electrical energy by means of dynamo machines, or in the reverse process of obtaining motive power by means of electro-motors, certain losses are unavoidable. Within the last few years these losses have been very considerably reduced, as the nature of the various prejudicial actions has been gradually ascertained; and there are now dynamo machines whose efficiency of conversion is considerably over 90 per cent., or which reproduce as electrical energy in the conductor more than this percentage of the mechanical energy expended on them. This is undoubtedly a satisfactory result, although, on account of the probable incorrectness of the electrical units, not quite so satisfactory as it appears to be, as was pointed out by Professor Adams in his inaugural address to this Society. This question of the correctness of the electrical units is one of practical commercial importance, as well as of scientific interest, now that electrical energy is becoming a marketable commodity. Although it may not be easy always in practical work to obtain measurements correct to 1 per cent., the recognition of the inaccuracy of the ohm, which, according to Lord Rayleigh's and

Mrs. Sidgwick's experiments, appears to be 1·33 per cent. too low, will make correctness possible, and will at least bring the average of even rough readings so much nearer to the truth.

In dynamo machines the various causes of loss, or waste of energy, which appear as heat "in the wrong place," putting aside air-friction, and friction at the bearings and commutator or collector, may be divided into two portions—

(1) That in the iron of the armature cores and of the field-magnets; and

(2) That in the conductors of the armature and of the field-magnets—using the word conductors as meaning the copper which is required to conduct.

The heating of armature cores where iron is used, and of all moving portions of a dynamo machine, is due principally to the generation of electric currents in the metallic masses used in its construction. These currents, eddying in closed circuits within the machine, have been called "Foucault currents," since Foucault was one of those who investigated their action. Although there is no real difference between such currents and those which are generated in the conductor proper, the name has been retained and largely used, for the reason, no doubt, that it is convenient to distinguish by a name these useless currents, in the iron and elsewhere, from the useful currents in the conductor. But as Faraday was the discoverer of these currents, and described them fully in his explanation of Arago's "Rotations," and as Foucault's chief contribution to the knowledge of their action consisted in showing that they caused heat, does it not seem a little inconsistent to call them "Foucault" currents? The name "eddy-currents" has been given to them, I think by Professor Silvanus Thompson, and, as this name seems a very apt one, I hope I shall not be doing wrong in using it here.

The prejudicial actions of eddy-currents in the iron cores of armatures are of several kinds. In the first place, heat is produced in the iron, and this heat is of course developed in accordance with the law, $H = C^2 R$, although, if it were necessary to ascertain what in any particular case the current or resistance is, it would probably be found a far from easy task.

These currents, besides heating the core of the armature, and so causing waste of energy, are in such a direction as to tend to demagnetise the ring or drum, so counteracting to some extent the inductive effect of the field-magnets; and in this way, and by direct induction—if it is permissible to consider the two actions as distinct—they reduce the useful currents in the wire, or lessen the E.M.F. developed.

But as the eddy-currents reduce the useful currents, so are they in turn reduced by the induction of the latter. These two currents are therefore to a certain extent mutually engaged in a damping or reducing action. Thus the generation of eddy-currents and the heating of the core will be least when the greatest current is being generated in the conductor, and greatest when the least current is flowing.

In the explanations so frequently given, in the early days, of the action of the Gramme magneto-machine, it was customary to show that with an open circuit the armature might be rotated without any work being done, save in overcoming the usual friction, and no reference was made to the heating of the core which takes place under such circumstances, although this heating is greater than when a large current is being generated.

It is sometimes supposed that the rise in temperature of the core is largely the result of conduction from the wire, but experiments that I have made show that the core is hottest when only a very small current, or no current at all, is being generated; and this in spite of the fact that the heating of the wire is proportional to the square of the current; and that the heat of the core, when a current is being generated in the wire, is in part due to its own eddy-currents, and partly to conduction from the wire.

This explanation accounts for the circumstance with which those who have had experience of the use of dynamo machines for charging secondary batteries are doubtless familiar—that, apparently in opposition to the law that the heat is as the square of the current, the charging machines often heat most at the end of the run, when giving the least current. It may have been noticed that during the early portion of the period of charging these cells, when the opposing E.M.F.—which acts in most

respects as a resistance—has been comparatively low, the temperature of the machine has not become high, although a large current may have been produced; but as the process continued, and the charging current has been reduced by the increased opposing E.M.F. of the cells, the machine may have become unusually hot. A portion of this heat—that in the field-magnet coils—is caused by an increase of the potential at the terminals determining a greater flow of current through these coils; but it will be found that the heating of the armature is from the cause already mentioned, viz., an unchecked development of the eddy-currents.

Where the increased strength of the field, resulting from the rise of potential at the terminals, prevents any noticeable diminution of the current, the armature core will become hotter.

Unless an increase of current accompany an increase in the field strength, the temperature of the armature core will be raised.

As a rough practical measure, in cases where the dynamo has not been too well designed with a view to the repression of eddy-currents, it is well to insert a resistance in the shunt circuit of the field, and to decrease the forward lead of the brushes in accordance with the retreat of the "diameter of commutation" resulting from the decreased current. This will economise the energy used in the shunt circuit, as well as the horse-power absorbed, without further diminishing the current.

In compound (shunt and series) dynamos the effect of the eddy-currents will be to reduce the economy of the machine as the work is reduced. In such machines, as the external work increases, there is necessarily a rise in the "electrical efficiency," or quotient of the electrical energy expended in the external circuit divided by the total electrical energy; because the increase of work is proportionately greater in the external than in the internal circuit. But besides this gain in the "electrical efficiency" of compound machines as the work increases, there is a further gain in the "efficiency of conversion," from the fact that the eddy-currents are proportionately, and perhaps absolutely, a cause of least loss when the current in the conductor is greatest.

The generation of eddy-currents in armature cores, it is

perhaps needless to say, is a good deal influenced by the electric conductivity of the iron employed in their construction; but to attempt to reduce these currents by using iron of high resistance would not be advisable, the low magnetic capacity of such iron being a much more objectionable feature than the low resistance of good soft iron. That the resistance is inversely as the magnetic capacity, and that this relation is a very complete one, has been shown by Professor Hughes, from experiments with various kinds of iron and steel, in his recent invaluable papers.*

The consideration of the action of eddy-currents may seem to point to the desirability of dispensing with iron in building armatures; but where proper precautions are taken to diminish such currents to a minimum, and to get rid of the ill effects they may produce, the advantages obtained by its use appear to be so great as to much more than counterbalance the disadvantages.

Eddy-currents are not confined to the iron of the armature. They are developed in all moving metallic masses which come within the magnetic field, as well as to a small extent in stationary metallic masses other than the field-magnets, which are influenced in some machines by the moving magnetism of the armature, and by the slight waves or undulations of the field-magnetism acting inductively on them. In this latter case they act as if they had a small movement in a perfectly steady magnetic field.

* The following is a portion of a table by Professor Hughes (Proceedings Institution of Mechanical Engineers, January, 1883. Proceedings of the Royal Society, December, 1883).

I have added the columns showing the percentage rise of resistance and fall of magnetic capacity.

Quality.	Electrical resistance per mile of ·040 diam.	Magnetic capacity (annealed).	Rise per cent. of resistance.	Fall per cent. of magnetic capacity.
Best Swedish charcoal iron, 1	191·52	525	—	—
„ „ „ 2	198·40	510	3·59	3·04
„ „ „ 3	199·62	503	4·23	4·37
Swedish Siemens-Martin iron	226·32	430	18·17	18·1
Puddled iron, best best	259·92	340	35·71	35·28
Best homogeneous soft Bessemer steel ...	266·52	291	39·16	44·58
„ „ hard „ „ ...	312·69	172	63·2	67·24
Fine crucible cast steel	350·08	84	82·8	84·0

Eddy-currents are further produced in the cores and pole-pieces of the field-magnets themselves by these slight waves in the current caused, amongst other things, by the momentary short-circuitings of coils by the brushes. Although the loss from this cause may be slight, it cannot be overlooked, since it has the effect of increasing the apparent resistance of the coils, and must be taken into account in winding the magnets. Thus in a shunt machine it will not suffice to calculate the resistance from the current and E.M.F. at the brushes, nor the current from the resistance and E.M.F. Similarly, in a series machine, the fall of potential in the field-circuit cannot be found by multiplying the resistance by the current. These various losses, each perhaps insignificant in itself, amount in the aggregate to something very considerable; and, as dynamos are already highly efficient, it is not in large and important points that improvement is to be looked for, but in details, by very small percentages saved here and there, in matters which may seem negligible.

The loss of energy taking place in the conductors of dynamo machines, and appearing as heat, is caused by self-induction and by the passage of currents through these conductors. That which results from the latter cause, as in all conductors, is expressed in heat units, according to Joule's law, $H = C^2 R t \times 0 \cdot 24$, where t is time in seconds.

In considering the proportions to give to conductors, Sir Wm. Thomson showed that the proper and most economical size is that which makes the annual outlay equal for interest and depreciation of the copper, and for the power wasted in heat. As was pointed out recently before this Society, the cost of copper being only one, and perhaps not the most important, of the charges connected with electric mains, Sir Wm. Thomson's rule could not be applied as it stood. So also in the internal circuits of dynamo machines, to which this law would perhaps apply more aptly than to external circuits, practical considerations of space and weight in most cases necessitate the reduction of the section of the conductor much below the point that strict attention to economy of energy would dictate.

This is especially the case in armatures, and the loss in this

part of the machine is therefore not inconsiderable. An additional waste of energy results also from the increase in the resistance of the conductor caused by the rise in its temperature, unless precautions are taken to prevent it. When it is remembered that in the armature conductor of some dynamo-machines a rise in temperature of 140° F., or 78° C., is by no means uncommon, and that few machines work at less than a rise of 90° F. or 50° C., it will be readily seen that this additional waste is not inconsiderable.

Clerk Maxwell stated that pure copper varies in resistance about 0·388 per cent. per degree C.; therefore a rise of 78° C. indicates an increased waste in the armature of 30 per cent., and a rise of 50° C. a similar increase of more than 19 per cent. On this account dynamos should always work in well-ventilated places.

Theoretically it is no doubt true to say that it requires an expenditure of energy to magnetise an electro-magnet, but none to keep it magnetised. But as it requires an expenditure of energy to keep the magnetising current flowing, the foregoing theoretical truth is rather a disappointing one.

In the field-magnet coils, as space is not usually so valuable as in armatures, the difficulties to be overcome in diminishing waste of energy are not so great as in the latter. Still there remains the stubborn fact that electro-magnetic coils must be made of a metal which objects to having electricity forced through it, and makes its objection felt by exacting a toll from every passing current.

It is not a difficult matter to obtain copper for electrical purposes of very high conductivity; but when 95 or 96 per cent. conductivity is spoken of, we are perhaps wont to imagine that we are very near the perfection of electrical conductors, instead of being only near the standard of the best electrical conductor known, and probably the best that can be hoped for.

Loss by heating in field-magnet coils is unavoidable, it being unlikely that makers of dynamo machines will revert to the use of permanent magnets, unless Professor Hughes, who exercises a veritable magician's control over any bits of iron or steel he takes into his hands, can find some quality and temper of steel, or some

method of magnetising, capable of giving results sufficiently powerful and permanent for the purpose.

But the fact must not be lost sight of that this loss by heating may be reduced to moderate proportions, if space is available. The magnetising power of a solenoid is proportional to the product of the current and convolutions, it being assumed that the iron core is of suitable dimensions. It is therefore a matter of indifference what metal is used for the wire of an electro-magnet, so far as magnetism is concerned—a fact, I think, first pointed out by Lenz, Faraday having, however, previously shown that the intensity of the magnetic field of a single wire carrying a current depended on current alone.* The resistance does not enter into the question at all, except from the point of view of economy. It is of course very necessary to regard the question from this point of view, the object being to obtain the maximum of magnetism for the minimum of money, by so proportioning the conductors of the field electro-magnets as to reduce as much as possible, or to the limit fixed by Sir Wm. Thomson, the waste in heat. Whatever is saved by preventing the production of heat is a gain to magnetism, or, in other words, electrical energy may be so employed as to give either heat or magnetism, but not both; which brings us back to the theoretical truth, that it costs nothing to maintain an electro-magnetic field.

Having suitable cores in the first place, and the magnetising product of current and convolutions being known, the question to be decided is one of commercial economy; subject to which, the object in series machines being to economise energy by reducing E.M.F., the necessary excitation of the magnets being obtained with the lowest possible resistance, the sectional area of the conductor being increased, the length being unaltered, or only increased to such an amount as is necessary to prevent a reduction of the current-convolutions. The best way to utilise the space available for electro-magnetic coils would be to employ a conductor of a square or rectangular section, thus avoiding the loss of space between wires of circular section; but except in the case of large

* "Exp. Res.," Vol. III., p. 400.

conductors this is at present usually impracticable, as the thickness of cotton or other insulating material found necessary is so great as to reduce the advantages that might be obtained by the use of such conductors. It is to be hoped that the growing demand for suitable conductors will lead to some improvement being effected in this respect. If it is not considered too great a refinement in economy of space, I would suggest that a small naked copper wire be wound in the helical space remaining empty when the ordinary wire is coiled. Such a wire need have no insulating covering, as the necessary insulation will be provided by the large wires on all sides of it, and the two wires would simply be soldered together at the ends of the coil, thus placing them in parallel circuit. The actual size of such a wire is found by multiplying the diameter of the ordinary wire with its covering by ·1547. The magnetising current in shunt machines, where space is available and copper is not stinted, may be reduced to a very small one by increasing the sectional area of the conductor, and its length, in such proportions as to keep the product of current and convolutions unaltered. Thus, in a shunt machine the minimum of heat-waste in the fields implies the minimum of current in the same: in practice, of course, it is sufficient to get this down to satisfy Sir Wm. Thomson's law.

I may point out that as with leads, so with dynamos—those which are economical in one place may be wasteful in another, according to the conditions of working and price of fuel or of horse-power; and that electrical engineers in the near future may have to consider, in designing a dynamo machine, whether it has to work on a pitbank at Newcastle, or to be driven by a gas-engine in a small village.

The PRESIDENT remarked that Mr. Mordey had presented a paper which touched upon many points of interest, and which, though comparatively short in itself, suggested points which should bring forth a good discussion from those who were interested in the perfecting of dynamo electric machines. The efficiency of measuring-instruments was fast becoming of the first importance, and the question mentioned in the paper as to the

way in which the efficiency of machines was dependent upon the accurate measurement of resistances, and on accurate standards, was of the first consideration. He had pointed out in his inaugural address that all efficiencies of machines obtained from experiments made up to the present time were more than 1 per cent. above their real efficiencies, and that this error arose from an error in the unit of resistance. The heating of armature cores, questions of the internal and external resistance of circuits and the production of Foucault currents, were points upon which the meeting would derive benefit from hearing the experience of those who were in a position to speak on these subjects. Professor Hughes would no doubt be able to say something as to the magnetic capacity of different kinds of iron, which was a matter of first importance in the construction of dynamo machines.

The law laid down by Sir Wm. Thomson in regard to distribution had not received sufficient discussion, but the opportunity presented itself that evening to treat it from its commercial aspect. Professor Hughes was said by the author of the paper to have exercised a magician's control over iron and steel; and, according to Professor Hughes himself, that control had been attained entirely by the rough treatment to which those metals had been subjected. Makers of dynamo machines and others would be interested to learn some of the secrets by which these forces of nature were controlled.

Mr. Crompton said that Mr. Mordey in his paper contented himself with pointing out the faults which existed in dynamo machines, but did not suggest any remedies. No doubt the question of the heating of the iron cores of the armatures of these machines deserved the most careful consideration, but he would ask Mr. Mordey and Professor Hughes if they could give any trustworthy data to prove that these heating effects were produced solely by the eddy-currents mentioned by the former.

The remedies hitherto adopted to prevent these heating effects had been all directed against eddy-currents—that is to say, to divide or split up the cores into as thin laminæ or wires as possible, and to insulate these laminæ or wires from one another, so as to limit the course of the eddy-currents as far as possible.

But after all these precautions, heating effects still become manifest in the cores, and to such an extent that they cannot be explained by eddy-currents, unless these eddy-currents had a far higher E.M.F. than it is reasonable to suppose that they possessed.

For his own part he considered that a great portion of the heating effects was probably due to *the molecular movements within the iron itself;* and he anxiously waited for Professor Hughes' opinion on this point.

If this opinion be correct, it was evident that the remedy lay in the use of the most suitable iron, such as Swedish charcoal iron, and with the iron most carefully annealed.

Professor Hughes had pointed out that if these conditions were observed the molecular movements of magnetisation and demagnetisation took place with the minimum expenditure of mechanical work; and it is probable that the heating effects would be similarly minimised.

He did not deny that the heating might probably be partly caused by eddy-currents; but it was probable that the truth lay between the two, namely, that both causes of heating were originally present.

He had made considerable purchase of Swedish iron, with a view to carry out experiments to obtain information on this point, and would soon be able to relate his experiments with that material. He disagreed with Mr. Mordey that the question of the best material to be employed for the cores of the field-magnets was simply one of bulk and weight. This might be true to a certain extent for those cases where the total size or weight of the machine was a matter of no moment; but in very many cases, such as the dynamo used for ship-lighting or tramcar motors and the like, any increase of weight was a serious matter.

Professor Hughes had stated that the magnetic capacity of ordinary cast-iron was only about half that of the best annealed charcoal iron; therefore machines with cast-iron magnets would be nearly twice as heavy as those with wrought-iron ones. In addition to this, the increased bulk of the core would require a greater length of wire and increased resistance in the field-magnet coils.

The increase of the armature resistance by 30 per cent., due to the heating of the coils mentioned by the author, would be a very serious matter in the case where the resistance of the armature was already considerable, as in the earlier types of machines, where the loss from this cause often amounted to 20 or 30 per cent. of the total electrical out-put; but in machines of more modern design, wherein the loss has been reduced to 3 or 4 per cent., a 30 per cent. increase due to heating of this small amount, making the total loss up to 5 per cent. at the outside, is not worth notice. Good mechanical construction was of such paramount importance that it was often necessary to sacrifice radiating surface and ventilation in order to obtain it.

The copper on the armature of dynamo machines carry much heavier currents per square inch of section than any other conductor employed in commercial operations. Four thousand ampères per square inch pass through the wires of small section wound on the armature of high-tension machines, without unduly heating them or damaging the insulation.

Conductors of square or rectangular section, as suggested by Mr. Mordey, had been many times tried, but the difficulties in their use chiefly rose from the tendency which wire of this section had to twist in the drawing. Moreover, the insulation was liable to cut on the sharp corners; but he hoped that these difficulties would be overcome.

Referring again to the quality of the best iron to be used in dynamo machines, he pointed out that one difficulty the makers had to contend with was the fact that real charcoal iron had been superseded throughout the country by mild steel, which, from its strength and ductility, was applicable to all the purposes (except magnetic ones) for which charcoal plates had been hitherto used. Manufacturers in all good faith supply mild steel when charcoal iron plates are specified; but this mild steel has a low magnetic capacity, and should be carefully avoided.

Mr. J. E. H. Gordon said that he quite agreed with Mr. Crompton that the heat was produced by magnetic changes, and not by electric currents.

Professor D. E. Hughes begged to be excused for not making

any remarks upon magnetism, seeing that on the following evening he had to deliver an exhaustive paper on the subject at the Royal Institution.

Professor G. FORBES said that no doubt, after more mature deliberation of the paper, there were many points upon which something might be said. He simply, for the moment, desired to get information, which no doubt Mr. Mordey would be able to give. But, as with other speakers, he would first make a remark about wires of square section. He was glad to hear that Mr. Crompton and others were interesting themselves in the matter. The loss of space was equal to 12·5 per cent. of the whole; and not only was such a loss important in dynamo machines, on account of the requisite insulation diminishing the full effect, but in special galvanometers and measuring instruments it was a matter of the greatest importance to get an extra mass of metal into the same space. He reminded the meeting of the recent beautiful and ingenious method announced by Mr. Wiedemann of insulating copper wire by a thin deposit of sulphate of lead, which was an absolute insulator, easily put on electrolytically, and which, by taking up the smallest possible amount of space, allowed a considerably greater quantity of metal to be used in the galvanometer.

The information he sought from Mr. Mordey was in reference to the field-magnets. He remembered a remark made some years ago by M. Niaudet, to the effect that in winding the wires upon the field-magnet of a Brush machine, a thin plate of copper foil was used as a foundation for the insulated wires. He was unaware whether the reason for that plate of copper being used had ever been published, and he asked Mr. Mordey to be good enough to say whether it was still used, and why. It was well known that the variations of the intensity of the current in the Brush machine were very considerable. Professor Ayrton had pointed out how that variation could be easily detected in an extremely interesting manner. He (Professor Forbes) imagined that, just as in an induction coil it was customary to put a tube of conducting metal round the iron core to prevent the iron being magnetised and demagnetised too quickly, so the copper in the

Brush machine was put as the basis for winding on the wires of the field-magnet to prevent the rapid magnetisation and demagnetisation of the core of the field-magnets, so as to equalise the electro-motive force uniformly. That seemed to him to be an extremely ingenious method of getting over what might have been a serious difficulty in the machine.

Mr. GISBERT KAPP asked Mr. Mordey whether he had found out that there was a maximum bulk of the field-magnets which must not be overstepped, if machines were wanted capable of being readily magnetised.

Professor W. E. AYRTON, in reference to Professor Forbes' remarks regarding the experiments which Professor Perry and he had conducted a year ago upon the inconstancy of the current produced by the so-called "constant-current" machines, was inclined to think that a great deal of the loss which occurred in certain machines was due to rapid fluctuations in the current, which an ordinary current-meter, no matter how dead-beat, would not detect. In order to produce a powerful machine, the Gramme pattern had been greatly departed from, until it more nearly approached the original Paccinotti machine, in the armature of which iron projections existed. The Brush machine armature had protruding pieces of iron in it, which immensely increased the number of lines of force which passed through the armature, and therefore very much increased the electro-motive force of the machine and its power for a given weight. But, on the other hand, their presence very much increased the want of steadiness in the machine; and the fluctuations could be detected by causing the so-called constant current to pass through a primary coil, and by measuring its effect upon a secondary coil.

If, for example, a certain current from a Gramme or an Edison machine be passed through the primary coil, then he had found that but little current was induced in the secondary; whereas, if the same current produced by a Brush machine was passed through the primary, a reverse current of quite measurable amount was induced in the secondary coil, by the fluctuations in the strength of the current in the primary.

Experiments led him to think that a good deal of the loss of

power in many machines arose from the presence of these rapid fluctuations in the current, which, not being observable, had been usually disregarded. Those effects could be diminished by increasing the size of the field-magnet. The larger the field-magnet, the less the disturbance produced. The disturbance was produced not only in the armature itself, but also in the field-magnets.

A proof that machines did not give nearly so steady a current as was supposed was shown by the sparking that occurred when accumulators were charged. In his opinion that sparking was due to the fact that accumulators had a constant back electro-motive force, while the charging machine had a fluctuating forward electro-motive force, which caused the current to undergo rapid variations during the process of charging, and hence there was a considerable loss of power at the brushes, which appeared in the shape of sparks. To entirely overcome that loss, it was necessary to have an armature in as many pieces as possible, with no protruding iron, so as to obtain the greatest uniformity of current, and so as to get the smallest possible effect in the secondary coil of the arrangement he had referred to. Mr. Crompton had mentioned the difficulty of obtaining charcoal iron, and, in addition, he would say that much of the iron called charcoal iron was really "coke iron." Enquiry into the subject showed that coke was extensively used in place of charcoal, though the name charcoal was retained; and manufacturers had informed him that it was very difficult now to obtain charcoal iron in England, at any rate in large quantities, unless special precautions were taken in the purchase. Certain firms from whom he had ordered specimens, on the expressed understanding that charcoal iron, and not coke iron, was wanted, wrote back to say that they could not supply real charcoal iron.

A Member asked Professor Ayrton whether the deflection of the instrument showing the effect on the secondary coil he had described indicated fluctuations of the current in the primary coil.

Professor AYRTON said the primary and secondary were insulated from one another. The current produced by the dynamo circulated through the primary coil only, and a current-meter

capable of measuring reverse currents, placed in the secondary coil, indicated by the mean strength of the reverse currents the magnitude of the fluctuations that occurred in the primary coil—the magnitude, in fact, of the "discontinuity."

If the current produced by the dynamo, and which passed through the primary coil, was absolutely steady, and was not merely what was commonly called a continuous current, then no deflection would be observed on the reversed current ammeter in the secondary circuit.

He believed that the employment of such a "discontinuity meter" as he had described, and which he had shown at one of his lectures at the London Institution last year, would be of great value to the buyers of dynamo machines; for he considered that an intending purchaser should not be content with merely being told the current and electro-motive force the machine would produce, but that he should make a point of ascertaining for himself the magnitude of the "discontinuity factor" in the machine in question. For it had been shown by his colleague and himself, that the danger arising from the use of a high electro-motive-force dynamo depended not merely on the magnitude of the electro-motive force, but also on the discontinuity factor of the machine, and that generators should be divided into four types of increasing danger,—(1) batteries or accumulators producing a perfectly non-oscillating current; (2) dynamos producing a fairly non-oscillating current; (3) so-called constant-current dynamos, but which really produced an oscillating current; (4) reverse-current dynamos,—and that if both the going and return wires were insulated from the earth an immensely greater electro-motive force was permissible with the first or second type than with the third or fourth.

Mr. W. M. MORDEY remarked that he was not a very ready debater, but he would do his best to answer the questions put to him. Mr. Crompton had asked whether he was quite sure that heat was really caused by eddy-currents, or was it not rather due to molecular movements in the iron. There was a good deal of proof that heat was caused by eddy-currents; was there any proof that heat was caused by molecular movement? If a penny were

spun between the poles of a magnet it became hot. That heat might be due to molecular movement, but it certainly was not due to the magnetising and demagnetising of the penny. If any molecular movements take place in non-magnetic metals when moving in a magnetic field, those movements, he assumed, must be due to diamagnetism, and he thought it very unlikely that this could be a cause of heating; therefore, in non-magnetic metals at any rate, it seemed to him there was practically only one cause of heat, and that was the generation of eddy-currents. And in iron, also, was it not easy to trace the heat to the induction of currents?

He had been misunderstood if Mr. Crompton thought he said it was not necessary to have good iron in the field-magnets. He had not said anything about the quality of iron for field-magnets; and for armatures, what he said was that it was not advisable to use iron of high resistance, on account of its low magnetic capacity.

With reference to the increase of waste in armatures brought about by high temperature, Mr. Crompton had said it was not a matter of very great importance that an increased loss of thirty per cent. in the armature should take place on this account. That increased loss took place when the armature heated up to a certain point, but it did not stop there, as it tended to shorten the life of a machine very considerably. When an armature reached a high temperature, and habitually worked under such a temperature, the life of that machine would be considerably shorter than if the heat could be kept down so that the percentage of loss was much smaller. It would be better to lose the thirty per cent. in ventilating the armature rather than lose it in increased resistance caused by heat, simply because the life of the machine would be thereby prolonged.

Mr. Gordon had also stated his opinion that the heating of armatures was a result of a molecular movement. It was not for him to question the opinions of such authorities as Mr. Gordon, but he was not aware of any experiments that proved that such movements were the cause of heat. Plenty of experiments can be described to show that eddy currents are produced in the iron, and it was shown by Foucault that these currents produce

heat in the metal in which they circulate. It was a matter of proof that currents were always induced in masses of metal, whether iron or not, which were revolved in magnetic fields, and it was placed beyond dispute that these currents caused great heat. These things were positively known; but where were the experiments proving that molecular movement was a sensible cause of heat in any armature?

If an armature consisting of solid copper, or even brass, were rotated, immense currents would be generated; indeed, in a powerful magnetic field it would probably be possible to melt a copper armature simply by the eddy-currents that were produced in it. Why, the fact that eddy-currents would be produced in a metal, by rotating it in a magnetic field, was the foundation on which dynamo machines were built; and the currents produced by such machines were simply eddy-currents properly controlled.

The fact that even when the iron of an armature is finely laminated and subdivided it still heats, did not, he thought, prove that the heat was caused by molecular movement, as no amount of subdividing could entirely get rid of eddy-currents. He ventured to suggest, as an experiment, that an armature of laminated brass or copper be rotated in a magnetic field. If lamination can stop eddy-currents in iron, it can do so also in brass; and, as in brass no molecular movement would take place, the heat which he felt sure would be produced could only be ascribed to eddy-currents.

Referring to Mr. Gordon's remarks on the subject of the heating of dynamo machines, he (Mr. Mordey) said that, unless the heat was got rid of as quickly as it was generated, the armature would heat more and more the longer it ran, and of course that was the reason why in some dynamo machines it was not safe to run them for more than a few hours.

It was a question of not only getting rid as much as possible of eddy-currents, but of getting rid also of the heat effects of the small eddy-currents that remained whenever iron was used. If heat accumulated in a dynamo, that machine was, he thought, defective. A well-designed dynamo machine ought to be capable of running for an unlimited time, so that it could be used if

required for charging accumulators or feeding motors in the daytime, and for running lights at night.

In his paper he had drawn attention to the fact that in the Gramme ring the heating of the iron was greatest when the external circuit was open, or when the current was small. Mr. Gordon thought this fact was common knowledge. He (Mr. Mordey) did not think it was at all well known, and in fact he had not seen it mentioned by any writer. As a proof that it was not by any means generally known, it was sufficient for him to say that Mr. Gordon did not himself appear to understand it, as was shown by his asking whether the heat at the end of a run in charging accumulators was not really due to the gradual accumulation of heat rather than to the diminution of current. Referring to this, he might say that he had seen dynamo machines giving their full current and developing their full electrical energy for many hours on a circuit of incandescent lamps, or through artificial resistance, and hardly getting sensibly warm. And he had seen the same machines giving a much smaller current on a circuit of accumulators and getting excessively hot. This, he thought, was a proof that the unchecked generation of eddy-currents, owing to the small amount of the damping or reducing action of the current in the wire, was really the cause of the increased heat in the iron. When he had stated to gentlemen who had great experience with accumulators that such and such a machine, which heated when charging accumulators, had run on incandescent lamps, giving a much larger current for many hours without heating, he had been met by dubious smiles, and by the remark that "the heat was as the square of the current." Exactly; the heat in the armature core was probably as the square of the eddy-currents, which currents were greatest when the least current was being generated in the wire, and the heat was therefore greatest then. This was simply the experience he had gained in his own work.

As to the increase in the section of the conductor, and his proposal to use a small bare wire, his paper was intended in a very humble way to point out a few small matters that might be improved in dynamo machines. Dynamos were highly efficient,

but if their efficiency could be improved by adopting simple precautions, was it not well to do so? Mr. Gordon had asked whether an increase of 1 to $1\frac{1}{4}$ per cent. was worth the trouble of winding an extra wire on. He (Mr. Mordey) thought it was; but the percentage would amount to more than that. Perhaps Mr. Gordon had taken the section of the conductor as that of the covered conductor. The increase in the section would be greater than would be shown by multiplying the covered diameter of the wire by ·1547—for instance, in the case of No. 8 B.W.G. ·165 inch in diameter, which was covered generally to about ·18 inch. ·18 multiplied by ·1547 would give the diameter of the wire to be put in, viz., ·028; but the increase of section would have to be found by adding a wire ·028 inch diameter to one ·165 inch diameter, not to one of ·18 inch diameter, and that would make the increase of sectional area amount to 3 per cent.

Referring to the enquiry of Professor Forbes respecting the employment in Mr. Brush's machine of a covering of copper on the field-magnets, he said that this was one of the many ingenious points in the machine. Any one who had had opportunities of studying the Brush machine must have admired more and more, as they became acquainted with the machine, the very great ingenuity—in the commutator, and generally in the whole machine—which was exercised, at a very early period in the history of electric lighting, in the design of that machine. He thought Professor Forbes was right in the supposition he had formed, or in the explanation that M. Niaudet gave him, as to the action of the copper.

Professor G. Forbes said that M. Niaudet had told him the fact, but neither of them could understand it at the time.

Mr. Mordey (continuing) said the action of that copper covering on the core of the magnet was simply to equalise and steady the current by checking the oscillations in the magnetic field; in fact, it had the same effect as the brass tube sometimes used in small induction coils for the purpose of enabling the intensity of the secondary current to be regulated—the variation in the current setting up a counter-current in the copper tending to

equalise the strength of the field-magnets and to reduce the extra current, or, should he say, the tendency to produce an extra current in the wire. It had also the important effect of reducing the potential difference at the terminals of the field-magnet circuit. The expense of putting such a copper sheath on the machine was small, and any gain obtained in that direction was worth having. It might be advisable to adopt the plan in some other machines, or rather it might have been if Mr. Brush had not done it in his own.

Referring to Mr. Kapp's remarks, he understood Mr. Kapp to say that he had advocated field-magnets of great bulk; if so, that was a misapprehension, and he was sorry to have so badly expressed himself as to have been misunderstood by two gentlemen that evening. He had really said nothing about the bulk of the field-magnets in dynamo machines, except indirectly, in so far as the length of wire on the coils was affected by their dimensions. He had no positive information that he could give, but he was under the impression that there was a maximum bulk, which if overstepped would certainly diminish the readiness of the machines to magnetise.

He did not know what time was occupied in the very large Edison machines, but he had heard that it took over three minutes for the magnets to become fully excited.

He remembered the experiments described by Professor Ayrton, for ascertaining the amount of induction that would be produced in a secondary circuit by the current from a Brush machine working through the primary circuit of an induction coil. He believed that in those experiments the coil consisted of two wires of about equal size and length, one over the other, or side by side; but he was unaware whether any experiment was made with an iron core in the induction coil. He also had tried some time ago experiments by joining up a large electro-magnet that was wound in two sections, so that the current from a Brush machine working through lamps, and therefore more unsteady than if it were working through a circuit consisting of artificial resistance, could go through one section, while another section was joined up to a Siemens electro-dynamometer. No current at all appeared,

neither could any effect be obtained when the terminals of the secondary coil were placed on his tongue. He did not know whether the Siemens dynamometer was too rough to give an indication; but, from experiments with cells, he was of opinion that his tongue would give an indication of any current that might be generated, especially if it were an alternating current. Perhaps the presence of an iron core in the electro-magnet reduced to some extent the alternate current induced by the Brush current; but the fact remained that he failed to detect even the slightest trace of current from that arrangement.

He thought that the undulation of the current from the Brush machine had been over estimated.

Theory showed that in working with high tension currents great danger existed from static discharge when undulations were taking place; but he had had experience of a circuit of 80 electric arc lamps in series, working with an E.M.F. of about 4,000 volts, and had stood on the damp floor of a workshop and had touched the circuit without having felt any of the serious results that might be expected.

Professor FORBES asked how many volts Mr. Mordey mentioned.

Mr. MORDEY: 4,000 volts. As to the sparking at the commutator of machines when charging accumulators being caused by the backward waves from the cells, he thought they all ought to feel obliged to Professor Ayrton for giving such a simple and easy explanation of the fact. He agreed also with Professor Ayrton that the number of segments ought to be as great as possible, or at least that it should be great enough. He had seen machines working with 15 steps or segments between brush and brush, with a very small amount of sparking indeed, very little more in fact than when sending the current through an ordinary circuit of incandescent lamps. The fields should be strong, no doubt, as Professor Ayrton had said; but in dynamo machines which had iron in the armature, strengthening the field beyond a certain point seemed to him likely to produce that great difficulty with Foucault or eddy-currents in the iron, which he had tried to show took place when the ordinary current produced in the wire was small.

The thanks of the meeting were unanimously voted to Mr. Mordey for his paper.

The PRESIDENT asked Professor Forbes to give the two short papers on "Breguét's Telephone" and on "Compensated Resistances," which he had promised in substitution for the paper set down in his name for the evening.

Professor G. FORBES: I must preface my remarks, Mr. President and gentlemen, by an explanation. Not long ago I was working out a number of problems and formulæ in the application of alternate currents, and arrived at some results which I think would have been interesting to the Society, and when our Secretary asked me to read them before you, I was perfectly prepared to do so. Unfortunately, when looking for my notes yesterday, I found that I must have left them in Scotland, where I had made them, and so I was unable to bring forward the paper which stands in my name. The notes contained some interesting problems, which touched upon three different matters of induction. The first part dealt with the self-induction of the machine itself—a matter which has been already discussed to a considerable extent by M. Joubert and others, and which admits of further development. The second part dealt with the induction in the cables, and in that case I applied the formula actually to some practical cases that had been suggested, and found that in such an installation as was proposed—to light up by alternate-current machines the whole length of the Suez Canal—it was quite impracticable, on account of the induced effects, which for such a length would be very serious; while in smaller installations the inductive charge of the conductor had very little effect in diminishing the current. The third part contained problems in the employment of alternate currents by means of what have been called secondary generators—that is to say, by the induced effects at the place where the current has been used.

I regret to say that I have not been able to bring the paper before you; but, finding that I was in an awkward position, and had promised what I was unable to perform, I offered to give two short communications.

The first of these is on "Breguét's Telephone."

At the time of my experiments I confess that I was ignorant of the existence of Breguét's telephone, and it was not until I had completed the experiments that I found that the work had been done before by Breguét. I say I was ignorant. It is very possible that I had known of it before; but I believe one is very often apt to retain knowledge in one's mind of something one has read of, and yet be perfectly ignorant of it, and thinks he is practically pursuing an original investigation. The instrument which I call Breguét's telephone is founded upon the instrument which was described by Lipmann, called the capillary electrometer. The phenomenon may be shown in a variety of ways. One of the easiest methods to show it is by taking a long glass tube and bending it into two glasses of dilute acid, and, the tube being filled with acid itself, a piece of mercury is placed in the centre of the tube. Then if one pole of a battery is connected with one vessel of acid, and the other pole of the battery is connected with the other vessel of acid, at the moment of connection the bit of mercury will be seen to travel to the right or left, according to the direction of the current. M. Lipmann explained the action by showing that the electro-motive force which is generated tends to alter the convexity of the surface of the mercury. The surface of the mercury, looked at from one side, has a convex form, which is altered by the electro-motive force set up when connection is made with the battery. The equilibrium of the mercury is dependent upon the convexity, and consequently when the convexity is disturbed the mercury moves to one side or the other. Lipmann also showed that if a tube containing a bit of mercury, and tapering to a point, is taken and dipped into acid, and then the tube filled with acid, on one pole of a battery being dipped into the tube and another into the acid the mercury will move up or down, showing similar action to that which I have just described.

Lipmann further showed the reverse effect, that if a piece of mercury be forcibly pressed, so as to alter the convexity of its surface, such as by bringing it into a narrower part of the tube, then there is an electro-motive force produced.

It occurred to me, and no doubt it did to Breguét also, that if

we speak either against the surface of the glass tube, and caused the tube to vibrate, or if the mercury were caused to vibrate in the manner I have shown, that we ought to be able to introduce a varying current in the wires which might have sufficient electro-motive force to produce audible speech in a Bell telephone. Further, the same instrument, since varying electro-motive force affected the drop of mercury and produced varying displacement, ought also to act as a receiving instrument, and should vibrate in accordance with the currents that arrive. My experiments have only been in the way of using the instrument as a transmitter; but Breguét, I find, used it as a receiver as well as a transmitter, though I am not aware that M. Breguét made any actual experiments so as to produce articulate speech. I presume that this was done, although I have not come across any description of the experiments, and it was for that reason that I thought possibly some account of my own experiments might be interesting to the members of the Society. The first tubes that I used were bits of glass tube about a centimètre diameter, and simply drawn out to a tapering point. I have the tubes here. The first experiment I tried was by tapping the glass tube so as to mechanically shift the position of the mercury, and by listening on the telephone for the effect. For a long time, at least an hour, I could get no effect at all. At last I got a sound, but could not understand how it was that at one time of tapping I could not hear, while at another time it was quite loud.

At the top I always got sound, but at the side I got no sound, although the mercury was shaking. I then tried to see how feeble a current was audible in the telephone. An assistant tapped the tube while I stood out of the way, and where I could not see. I got him to tap it gentler and gentler, and could hear the most feeble tap. A pellet of paper was next dropped from various heights down to an inch, and each tap was perfectly audible in the telephone. I tried many methods, and one, purely accidentally chosen, was a piece of glass tube which I had drawn out into a tube about 2 mm. diameter, and then nearly closed the end of it. I have that tube here, and you will see what an ill-shapen and ugly-looking tube it is, but it is one of the best tubes I ever got;

and finally, I found that small bits of thermometer tube, which were simply closed at their ends with a blow-pipe, gave very good results, and I was able to make them useful for various purposes. I then tried mounting a tube on the end of a speaking-trumpet and speaking to the mercury, but got no effect. In every place where I attached the glass tube itself to a sounding-board I got a very accurate reproduction. I put one on a piece of ferrotype plate, and that gave really the best result I ever got. The tube was fastened with sealing-wax, and with it I got excellent speech heard in a Bell receiver. I tried putting in a large number of these tubes, all in quantity, on the bottom of a ferrotype plate, but with no advantage. I have not yet tried putting them in series, one behind the other, so as to increase the electro-motive force, but I think that probably would be an improvement; of course it would require many vessels of acidulated water to dip into. The most distinct articulate speech was obtained from an ordinary ferrotype telephone plate, secured at the edges, and one of the glass tubes you see here attached to it. I think those are all the facts I have to mention in connection with this Breguét telephone.

The other communication I have to make is a very short one, but I think it may be of some interest to many practical gentlemen who are present. The subject is "Compensated Resistances."

Of course we are able to get standards of resistance with very great accuracy by using platinum-silver or other alloy. Every day new alloys are being tested, and give us hopes of still further advances in the constancy of the resistance with varying temperature. But some of these alloys are extremely expensive. It also happens that we want constant resistances not only as standards, but also for another purpose. It is becoming very generally the custom to measure differences of potential by high-resistance galvanometers, and the resistances of these galvanometers vary so seriously with the temperature, that it becomes a matter of great importance to have these potential galvanometers uniform in their action whatever the temperature may be. For this purpose Sir William Thomson makes his graded potential galvanometers of German silver wire, and others, I believe, have followed in his

footsteps. The resistance of German silver wire is much greater than that of copper wire; therefore, with an equal number of turns of wire of the same diameter, we get a greater resistance with the German silver than we do with the copper, which is always an advantage, but at a greater expense. It occurred to me that since we possess two classes of materials which vary differently in their behaviour with change of temperature, we might be able to produce a compensated resistance (just as we are able to produce a compensated pendulum) which, however the temperature varied, would always remain the same. The resistance of a copper high-resistance galvanometer being taken equal to R, and the resistance at zero temperature R_0, then

$$R = R_0 (1 + a\,t)$$

where t is the temperature (centigrade), and $a = 0{\cdot}0038$.

Now, if you put into the same circuit another resistance, say, the carbon of an incandescent lamp, you will have its resistance $r = r_0 (1 - a't)$, because the resistance diminishes with increase of temperature in the case of carbon. Then the whole resistance is

$$R + r = R_0 + r_0 + (a\,R_0 - a'\,r_0)\,t.$$

If we adjust the two resistances of the copper and of the carbon in such a way that they are inversely proportional to the constants a and a', then we shall find that the term $(a\,R_0 - a'\,r_0)\,t$ will be equal to zero, and the result is that we have $R + r = R_0 + r_0$, and that is a constant resistance. But before we are able to apply this, it is necessary that we prove that the variation in the resistance of carbon is a gradual variation. Now it has been supposed by many that carbon suddenly diminishes its resistance at the time it is raised up to the temperature of luminosity, and it was to test this that I undertook some experiments not very long ago, and I found, testing the carbon as prepared in incandescent lamps, that the change in temperature is a gradual change. I think it probable that this subject has been worked out by other people, although I have not been able to come across their work, and this communication to you may cause another "Breguét" to announce himself. The change is gradual, and in my experiments could be steadily traced from the temperature of the room in which I was

working up to that of boiling water, and I was able to arrive at a mean, from a good number of trials, of the value of $a' = 0{\cdot}0005$. For pure metals whose resistance increases with increase of temperature the value of a is about ·0038, and it follows that if we had a galvanometer of a resistance equal to 50 ohms, and we were to put into the circuit a resistance of carbon equal to 380 ohms, we should have a total resistance of 430 ohms, and that resistance would be tolerably constant. Certainly that resistance would be constant enough, between the ordinary variations of temperature that we have in this country, for rendering our potential galvanometer accurate, and it has the advantage that any ordinary high-resistance galvanometer can be transformed without rewinding it with German silver, as has been hitherto necessary. There is another curious remark that I ought to make, that if we put equal weights of copper and German silver in two galvanometers, then the resistance of the German silver one will be about thirteen times greater than the resistance of the copper one, and the current going through such coils will be less, and therefore the directive influence of the current on the needle of the galvanometer will be less. Therefore, although having the greater resistance in the case of the German silver wire is an advantage, having the feebler current effect is a disadvantage and defect. With the carbon introduced in addition to the copper, we get a galvanometer in which the resistance of the carbon and the copper combined is much less than the resistance of the German silver one, and yet the magnetic effect upon the needle of the galvanometer is still equal in the two cases. The copper-and-carbon galvanometer is really superior to the German silver one, and yet it has the advantage that the addition can be made to any galvanometer which at present exists.

The PRESIDENT: We have arrived at a late hour, and I am sorry that we have not time for a full discussion upon these subjects; still perhaps some members may like to make a few remarks.

Dr. J. A. FLEMING: The extremely ingenious arrangement of compensating conductors Professor Forbes has described to us, is

exactly analogous to the method of compensation adopted in the Edison electric-light-meter. The coefficient of variation resistance in metals is positive, in carbon negative, and also negative in the case of liquids. The method adopted in the Edison meter for recording the current, is by sending a portion of the current through a voltmeter in which two zinc plates in solution of sulphate of zinc are placed. The whole of the current cannot of course be passed through a meter, because its strength would prevent the uniform deposit of metal, and a shunt arrangement is adopted in which a fraction, $\frac{1}{800}$ or $\frac{1}{1000}$ part, is passed through the voltmeter, the main portion of the current going through a German silver strip. The resistance of sulphate of zinc, like that of carbon, decreases with an increase of temperature, and so, when an increase of temperature takes place, a greater current will pass through the voltmeter than that which it is adjusted to give at a lower temperature; and accordingly Mr. Edison has been compelled to devise some arrangement of compensating conductors which shall be independent of temperature. Mr. Edison puts in series with the voltmeter a small copper coil, and the value of this compensating resistance is obtained by an equation similar to that Professor Forbes has given on the board. By using the copper coil, the resistance of which increases with rise of temperature, in series with the zinc solution, whose resistance decreases with rise of temperature, the shunt circuit is the same in all temperatures. The value of the resistance of the compensating coil has to be calculated for each different meter in accordance with the distance of the plates.

Professor W. E. Ayrton: It is a little invidious to be the "Breguét" which Professor Forbes said would probably turn up, and Dr. Fleming has been the Breguét. I would add that the plan of combining platinum and carbon is the principle of the Gatehouse incandescent lamp.

Professor Forbes: Not in series.

Professor Ayrton: I think sometimes in series.

Professor Forbes: No.

Professor Ayrton: It may be so. A somewhat similar idea occurred to me some time ago in connection with high-resistance

galvanometers, but the difficulty has always seemed to me to lie in winding the carbon filament on the galvanometer. If you wish to maintain a deflection on a voltmeter with the least expenditure of energy, you do not want to add resistance to the instrument, and all the wire through which the current passes should be on the instrument, and should have the highest possible conductivity. I pointed out at the Physical Society some time ago that it is better to wind a voltmeter with copper than with German silver wire. The great difficulty in voltmeters and high-resistance galvanometers for commercial purposes is not a temperature error arising from the change of temperature day by day, but a temperature error arising from the heating of the coil by the passage of the current itself. If you wind a voltmeter with German silver instead of copper, as Sir William Thomson used to do, but I understand does not do so now, then, although the change in resistance from day to day would be very small, the change due to heat by the passage of the current you want to measure will be much greater, because the resistance of German silver is about thirteen times as much as the resistance of copper, whereas the change of resistance by a given increase of temperature is only 8·8 times as great in copper as in German silver. There is, therefore, a great objection to using any substance which has a high resistance in the coils of a voltmeter; in fact, on a voltmeter bobbin the very highest conductivity copper should be used, and not anything like German silver. If, however, the consumption of energy necessary to maintain a given deflection be unimportant, then a portion of the total resistance should be wound in the form of high conductivity copper on the voltmeter bobbin, and the remainder of the resistance necessary to be introduced to prevent the voltmeter shunting too much of the main current may well be made of thick German silver, and may take the form of a separate-resistance coil. In that case, where consumption of energy to maintain the deflection is disregarded, the carbon device of Professor Forbes may be adopted.

I hope we shall have the benefit of Professor Forbes' other paper, which unfortunately he could not bring before us to-night.

Professor G. FORBES: There is not much for me to say. As to the question of priority, I am absolutely careless in any

investigation of this kind. I do not take it to be a question of priority; I am only glad to think that other people have worked in the same sphere. The only thing is that we should not give too much praise simply to an idea. We have had the idea that a constant resistance could be obtained by carbon and copper, but until the variation in the resistance of carbon was proved to be continuous with change of temperature, that remained only an idea, and that is what my experiments have proved. Professor Ayrton has raised a question as to the use of copper, instead of German silver, for high-resistance galvanometers, and there is a great deal to be said on both sides of that question, but it is too late to enter into the matter now. Certainly I have seen many advantages in the plan that Sir William Thomson has adopted, and I have also seen that the adoption of a copper-carbon compound compensated resistance is really an advance on the German silver one of Sir William Thomson.

The PRESIDENT: It is unfortunate that we have no time this evening for any discussion upon the "Breguét Telephone," or for further discussion on "Compensated Resistances," but I am sure we are very much obliged to Professor Forbes for the two interesting communications he has made to us this evening in lieu of the paper which he had promised, and I will ask you to give him a hearty vote of thanks for them.

A hearty vote of thanks was accorded to Professor Forbes.

A ballot then took place, at which the following were elected:

Foreign Members:

Ezra T. Gilliland. | Wm. B. Varisize.

Associate:

James Burke.

The meeting then adjourned until Thursday evening, 13th March, 1884.

The One Hundred and Thirty-second Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 13th, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The following transfer was announced as having been approved by the Council:—Mr. W. Leonard, from class of Associates to that of Members.

The names of new candidates were announced and suspended.

Donations to the Library were announced as having been received from Mr. J. A. Berly, Professor Silvanus Thompson, Mr. J. J. Storrow (Boston, U.S.A.), the Cornell University, Mr. O. Schoeffler, and Mr. A. Hartleben, Leipzig.

The meeting accorded a vote of thanks to the donors for their presentations.

The following paper was then read:—

NOTES ON A TRAIN-LIGHTING EXPERIMENT.

By W. H. MASSEY, Member.

Little is known about the cost of lighting railway carriages by means of incandescent lamps, and these notes of an experiment made recently on the District Railway are offered by the writer in the hope that other members who may have had opportunities of observing similar trials elsewhere will be induced to bring forward the results of their experience, and assist in the discussion of a question which has not hitherto received the amount of attention it deserves.

When, about five months ago, Mr. Forbes, chairman of the Metropolitan District Railway, decided to have a train fitted with electric light, the writer feared that the price of the necessary machinery, etc., would be thought too high by those whom the new light would be most useful to, and that the working expenses might not compare favourably with the cost of lighting by

compressed gas; but he is now of opinion that the first cost of electrical plant may be even less than that of some kinds of gas apparatus often fitted to trains, and that the light itself can be produced very cheaply. Its introduction into railway carriages ought therefore to prove a commercial success, if makers of engines and dynamo machines can be relied on to supply machinery which will run for several hours at a stretch without being constantly seen to while at work, and not require any greater amount of cleaning up or adjusting than could be done by the men who are at present engaged in looking after locomotive-engines, etc., on "shed" days—that is, once a week.

No serious attempt has been made in this country to light trains efficiently by oil lamps; and, as it is universally admitted that the various systems of gas-lighting—notably those in which compressed oil-gas is used—are the most satisfactory and economical, it is only with these improved methods that the electric light need be compared in this short paper. But there is some difficulty in doing this properly, because a good deal of uncertainty exists in the minds of the majority of railway officials as to what is actually spent in lighting their carriages; and the writer was obliged to make many patient enquiries before determining, with some degree of accuracy, that gas of very high illuminating power,* and suitable for train lighting, cost not less than twelve shillings per thousand cubic feet. Representatives of one gas company have hinted in a vague way at being able to supply rich compressed gas at about $\frac{1}{10}$ of a penny per $\frac{8}{10}$-foot burner per hour; whilst members of another firm (who most obligingly furnished information as to practical details) seemed confident about being in a position to manufacture gas for even less than this; and it is not easy to reconcile the many conflicting statements which have been made. It is extremely doubtful whether the prices quoted were in every case really supposed to include interest on capital, depreciation charges, and cost of repairs, etc; so that, although the first-named figure (12s.) may

* Pintsch's best gas is nearly up to 50-candle standard, or about three times the illuminating power of the average London gas.

possibly be higher than it should be theoretically, the want of more exact particulars, from the gas-lighting people themselves, leaves us no alternative but to be guided by the experience of those who make the gas for use in their own carriages; and they find in practice that the cost of everything connected with the lighting exceeds 12s. per thousand, and that to well light a first-class compartment two one-foot per hour burners are required. According to this it would appear that for purposes of comparison the expense of gas-lighting may safely be put down at more than 12s. per thousand lamp-hours, for a light of eight or nine candles per burner, which is as much as the very best gas jets give, but which is far above the average power of such lights as travellers on the city railways are accustomed to,—it being the writer's intention to show that the electric light is superior to anything else, while at the same time there is a reasonable prospect of its proving cheaper in the end.

Before describing the electric-lighting gear employed in the District Railway experiment, the writer would wish it to be clearly understood that, if the plan suggested by him had been adopted, a small engine and dynamo would have been fixed on the locomotive, where steam could be taken directly from the main boiler; but the railway authorities and the contractors acting for the Swan Company thought it would be preferable to place all the machinery, with a separate boiler and a youth to stoke it, in a van by itself; and, as it was the writer's privilege merely to advise and to see that the best was done in the interests of everybody concerned, this latter arrangement was therefore carried out, a London, Chatham, and Dover parcels-van having been borrowed for the purpose.

The object of the experiment was to enable Lord S. Cecil, general manager of the District Railway, to judge of the effect of the light, and to form some idea of the cost of working it, with such machinery as could be got together at short notice; and it was decided that a practical trial extending over three or four weeks' regular work would be sufficient to show what was required, seeing that the engine and dynamo had already been subjected to a severe test at the works of the contractors, where they were

driven daily for from ten to fourteen hours during nearly five weeks, before the complete apparatus was erected in the van.* In addition to this, the whole plant was tried on several occasions previously to the train being put on the road, in order to ascertain if there were any likelihood of a breakdown, and, everything proving satisfactory, the trials might have been commenced about the middle of December last; but running in public was not attempted until some time after the Christmas holidays.

The relative positions of the dynamo, engine, and boiler, etc., are so clearly shown in the plan of the floor of the parcels-van (Fig. 1), that scarcely any description will be necessary, but it may be noted in passing that the same machinery could have

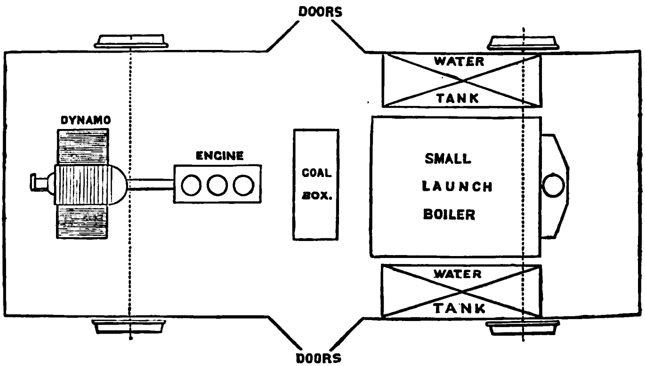

FIG. 1.

been carried in a smaller van, or arranged so as to occupy less floor space.

The total weight of the gear, including 1 ton of water in boiler and tanks, 2 cwt. of coal, and the driver, was under 3 tons, and the centre of gravity of the whole was about half-way between the axles of the van, the safe working load of which had been fixed at 3½ tons. The dynamo was a Siemens S D_1 self-regulating machine capable of driving 120 high-resistance lamps, but in the

* On one occasion the machinery ran without being attended to in any way for eighteen hours, and the engine-makers were quite willing to let it run night and day for a whole week if need be.

present case it was coupled direct to a Willans patent 3-cylinder engine intended to run at only 530 revolutions per minute, and at this speed the dynamo produced current enough for 52 Swan lamps (50 volts) of nearly 20 candle-power each. The steam pressure in the boiler was kept up to 120 lbs. per square inch, and with that the engine indicated rather more than 7 horse-power. This engine, being of a type often used in steam launches, was fitted with reversing gear and an extra lever, by moving which it could be worked either as compound or with simple expansion, at pleasure.

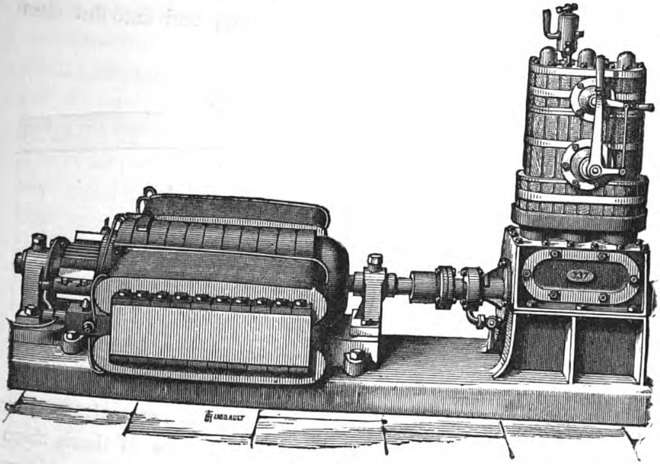

Fig. 2.

A perspective sketch of the dynamo and engine, bolted to one bed-plate as they stood in the van, is given in the accompanying figure.

All the arrangements being necessarily of a temporary character, the cables were just laid continuously along the roofs of the carriages, and roughly protected by wooden casings. No provision was made for uncoupling, because the Putney train, which was selected for the trials, is, like many others on the underground railway, seldom divided, and it is a question whether anything more than a common screwed socket between the

carriages would ever be required on this line; but on main lines, or in cases where it might be necessary to split up a train, self-acting spring connections could be fixed at each end of every carriage. The branch wires were led through short pipes, forming part of the lamp shades and holders combined, which were fastened to the ceilings by means of a nut. These fixtures were the most suitable that could be obtained at the time, and with certain modifications they would answer very well for experimental lighting, but for permanent work something neater and stronger must be designed.

Thirty Swan lamps were disposed as follows:—Three in each 1st class double compartment, one in each 2nd and 3rd class

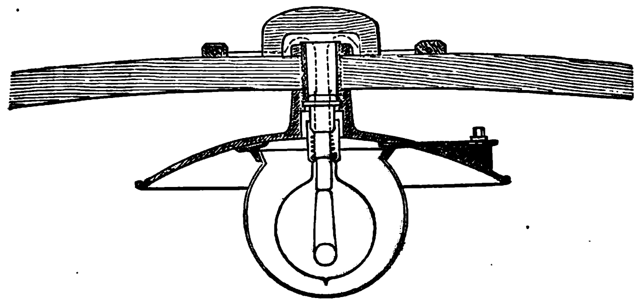

Fig. 3.

compartment of the train, and in the guards' compartments, as well as two in each of the tail lamps. Besides all these, there were not less than twenty lamps always burning in the machinery van, so that a fair estimate might be formed of the expenses likely to be incurred when driving the full number (about fifty) required for lighting most of the longer trains. It would perhaps be well to mention at this point, that in order to guard against the fusing of the platinum eyes, which it was thought might be caused by the incessant make and break of contact due to vibration when lamps are supported by rigid hooks, some very slight spring attachments were provided in the holders (*see* Fig. 3), and the only trouble of which persons in charge of the train complained was the being obliged to refix, during the early part of

each night's run, sometimes as many as three lamps which had slipped off hooks not strong enough to retain their shape. This is not a very serious thing, and it would be easy to prevent similar occurrences in future; but attention has been called to the matter so forcibly as to convince the writer that the great object to be attained in train lighting is to keep the lamps and everything else going as long as possible without the necessity of any one looking after them.

Mr. Vincent Allpress, one of the writer's pupils, visited the train daily from the 3rd of January, when it commenced to run regularly between High Street, Kensington, and Putney Bridge; and he reports that no hitch of any kind occurred in the working of the machinery for more than a fortnight, except on one evening when a playful passenger removed a lamp shade and globe bodily, and so caused a short circuit which affected the whole of the lamps for nearly two minutes. The hours of running were from 3 to 11.30 p.m. each day (Sundays excepted), and at the end of 15 days' steaming the boiler needed to be washed out, so the train went into the yard on the 21st January for this to be done, and the opportunity was taken at the same time to make the lamp-shades more secure. The train was put on the road again four days afterwards, when certain defects in the main wires, due partly to accidental, and partly to wilful damage done to them, at once began to cause annoyance, and it became necessary, after running in a patched condition for three days, to withdraw the train for repairs until the 1st of February, on which day it again went into regular running, the hours of lighting having in the meanwhile been altered to 6½, viz., from 5 till 11.30 p.m., as the days lengthened. The machinery continued to work admirably up to the time when the experiment ended, on the 16th of February, there being absolutely nothing to record against the lighting but the occasional jumping off of a lamp or two as explained before, and the trifling inconvenience suffered if this took place when it was not possible to refix them immediately.

The general opinion of the passengers on the line, and of those who went purposely to see the train, was that the light appeared too bright; but as it invariably happened that after

sitting in the carriages for a short time these same individuals believed the light to be much less dazzling than when they first saw it (while as a matter of fact there had never been the slightest change in the candle-power of the lamps), the writer thinks that imaginary objections only could be raised on the score of brilliancy, saving always that made by the managing director of an oil-gas company, who declared that the light was "far too good to pay."

Apart from the success of the experiment, regarded purely as an application of electric light on a somewhat larger scale than usual to railway trains, the information obtained during the preliminary and other trials ought to bring about a settlement of a few vexed questions relating to the lighting of carriages, although room may be left for conjecture as to the satisfactory working of specially-designed machinery in the varying conditions under which it would be applied to trains. One important fact at least seems to be fairly established (as careful tests, made by the writer, and by others independently, prove), viz., that by evaporating 211 pounds of water into steam at a pressure of 120 pounds per square inch, it is possible to keep 50 lamps, of 16 to 18 candles each, going for one hour; and, as the efficiency of the dynamo used was rather low, on account of the reduced speed of driving, it is probable that slightly better results might be obtained in cases where the machinery is more perfectly adapted to the work. Taking matters as they are, however, $4\frac{1}{4}$ horse-power was used in the lamps and leads, over 30 per cent. of the effective power of the engine wasted in the machine, and nearly 15 per cent. of the gross power lost in engine friction, so that the indicated power (7·175) was obtained for about 30 pounds of water per horse per hour, which is a fairly economical performance.* Once, when the

* The water tank measured 3 feet $11\frac{1}{2}$ inches × 1 foot $10\frac{3}{4}$ inches, but the actual weight of water per foot in depth was 452 pounds, and the amount of water used was $5\frac{9}{16}$ inches on a average.

The contractors made a trial of the same engine and found the consumption of water to be only 28 pounds per horse-power, but the writer was not present at that particular trial.

engine was worked on the simple or non-compound principle, the consumption of water was nearly 40 pounds per horse-power per hour, which may be taken as showing the advantage of fully expanding steam even in small high-speed engines.

The amount of fuel required to evaporate 211 lbs. of water into steam at a working pressure of 120 lbs. per square inch, will vary with the style of boiler employed, and it would be misleading to base any calculations upon the coal bill in this experiment. The small launch boiler lent by the contractors was good of its kind, but the consumption of coal was rather heavy, the daily average for lighting fire, getting up steam, and running for $8\frac{1}{2}$ hours being as much as $3\frac{1}{4}$ cwts., and for $6\frac{1}{2}$ hours' work about $2\frac{3}{4}$ cwts.* If steam were taken from the locomotive boiler of the train, the quantity of coal burnt per hour would be under 24 lbs. (that is, supposing that only 9 lbs. of water are evaporated per pound of coal), and this, with coal at 17s. to 18s. per ton, would give about 3s. 9d. as the cost of fuel per thousand lamp-hours. The cost of the water itself would be less than 3d. for the same number of lamp-hours, and the oil and cotton-waste may be put down at 1s. 3d., although this is rather a high figure if proper arrangements are made to catch the oil and to use it over and over again.

One of the chief items in the expenditure may be expected under the head of "lamp renewals," but the behaviour of the Swan lamps was rather exceptional, so that it would be better to fall back upon experience gained elsewhere than in this experiment. The number of lamps which fail daily in ordinary lighting is found generally to be a pretty constant quantity during the whole time an electric plant is being used, and it is not safe to reckon upon having to make less than one renewal per thousand lamp-hours from the very start, if the lamps are kept up to full brilliancy; but in this case they were not forced beyond 18 candle-

* In justice to the driver, it should be stated that the coal was not always first-rate, and that nearly twice as much coal was burnt on some days as on others, although the work was exactly the same.

power, as that was found to be quite sufficient to give a thoroughly good light; and the result is, that among lamps which have been in frequent use ever since the machinery was first tested at the works of the contractors in the early part of last October (most of these being subsequently used throughout a series of private trials in the railway yard), the filaments of only seven of them gave way in actual work. That there should be such a small number of failures is undoubtedly due to the lamps being worked below what is intended to be their normal power, and it would seem to be desirable to treat all lamps used for train lighting in the same way; for, even if the value of the energy thus wasted exceeds that of renewals which would otherwise be required, it is a great advantage to be able to run lamps for some considerable time without having to pay much attention to them. In estimating the probable working expenses, it would scarcely be right to assume that the lamp returns would be more favourable than usual, but taking into account the breakages from every cause, and having regard to the wholesale prices of lamps now in the market, the cost of renewals should never exceed 2s. 6d. per thousand lamp-hours.

The following particulars about the lamps may prove interesting:—

October, 1883. New lamps sent to Messrs. Willans & Robinson, Thames Ditton ...	50
December, 1883. New lamps sent to District Railway, Lillie Bridge	100
Total new lamps issued ...	150

Stock, February 16th, 1884—

New lamps still at District Railway stores ...	69
Lamps in place—carriages and van of train...	51
Lamps in van, but not used (defective platinums)	8
Lamps borrowed, but not returned	1
Lamps missing and not accounted for ...	1
Balance...	130

Lamp renewals—

Lamps broken accidentally at Thames Ditton	5	
Lamps broken accidentally at Lillie Bridge (fixing and repairing) ...	6	
Lamps defective and burnt up on testing circuits	2	
Broken and defective ...	—	13
Filaments failed in actual work at contractors'	3	
Filaments failed in actual work on train	4	
Failures during work ...	—	7
Total lamps destroyed ...		20

On bringing together the various items already given, it will be seen that the working expenses of the electric light are—

	s.	d.
For coal, per thousand lamp-hours	3	9
„ water „ „ „	0	3
„ oil and waste „	1	3
„ lamp renewals „	2	6
In all	7	9

to which must be added interest on capital, and charges for depreciation and repairs.

The first cost of fitting up a train with 50 lamps and suitable machinery, etc., would be about £170. An engine and dynamo, with hinges and springs, can be fixed to the locomotive of any train for £120, and £1 per lamp is a very fair allowance to make for all the rest of the electric light work, supposing trains to be fitted out in any great quantity; so that some £40 per year ought certainly to cover charges for interest and repairs, etc., even on the underground railways, where the average number of working hours for a locomotive is over 5,000 per year,* which, being equal

* Locomotive engines are usually worked for six days out of seven, and the taking into account of the working hours per year is equivalent to allowing interest, etc., upon $\frac{7}{6}$ of the plant actually in use at any time for lighting the carriages of the trains.

to 3s. 3d. per thousand lamp-hours, brings the total cost of the electric light to about 11s., or less than the price of gas-lighting. This, be it remembered, is no fanciful estimate, and it provides for lamps of double the illuminating power of the very best gas lights now used in railway carriages.

Supposing that 50 incandescent lamps of 10 candles each were put in the place of the 18-candle-power lamps alluded to above (in which case the trains would be very little better lighted than they are at present by means of gas), there would not be any great reduction in the total cost, because the capital outlay would be almost as large as before, and only a slight saving in fuel effected. But supposing it were found practicable to make, say, 30 good lights of 18 candle-power do the work of 50 ordinary gas jets of 8 or 9 candles at most, it would then be possible to light a train for nearly 30 per cent. less than was stated above; for, although the cost per thousand lamp-hours would be the same (the coal bill being less, but the charges for interest, etc., greater), the rate per working hour would be cheaper. Assuming, however, the higher estimate to be correct, railway travellers would do well to bear in mind, that the money paid at any railway bookstall for a candle by which one single passenger is enabled to read for nearly three hours is enough to cover the cost of lighting a whole compartment for fifteen hours by means of an 18-candle-power incandescent lamp, and that a passenger buying a 7s. 6d. reading-lamp invests as much as would keep the same compartment well lighted for two hours per night all the year round!*

This applies only to cases where the machinery is driven by steam taken directly from the locomotive boiler; and it is for engineers to consider whether such an arrangement can be depended upon in practice, having regard chiefly to the importance of being able to run without fear of a breakdown. Speaking as a practical engineer, the writer thinks that the electric light worked in this manner would be no more likely to

* A great number of railway reading-lamps is sold in a twelvemonth, and the writer has a good reason to believe that one large London house alone disposes of about sixty thousand twopenny candles per year.

fail than air brakes which are driven by small engines, and upon which the safety of so many people depends; and he sees no reason why specially-designed machinery should be any less reliable than the main engines of the train itself, or that it should require any attention from the drivers beyond turning on the steam whenever the light was wanted, and keeping the lubricators charged with oil. On underground railways where locomotives are worked some fourteen or fifteen hours per day with only very short intervals of rest, the machinery would be tried to the

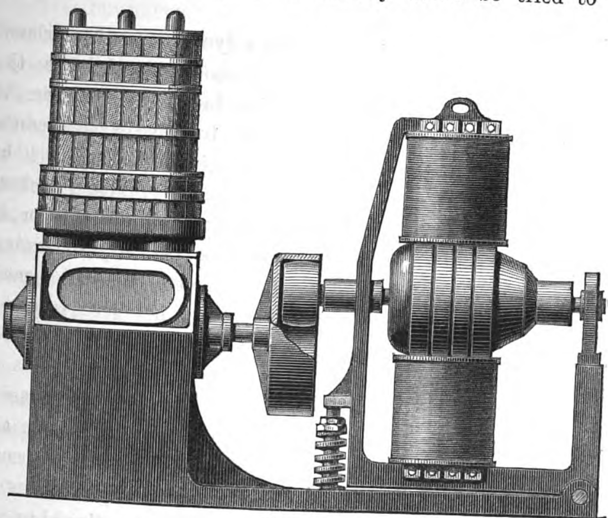

FIG. 4.

utmost; but on main lines the work is much lighter, and failures would consequently be rare. If ever secondary batteries can be made to stand railway work, it might be possible to guard against breakdowns and to provide for the splitting up of trains into numerous portions without these being left in darkness at all at badly-lighted stations; but such batteries are not yet to be had.

An engine and dynamo, arranged as shown in Fig. 4, can easily be placed in a wrought-iron casing 4 feet 6 inches long, 3 feet high, and 15 inches wide, and, supposing some means of ventilating the armature to be provided, it is extremely

improbable that any accident would happen to this machinery if carried on a locomotive.

Lighting by means of primary batteries has been attempted in several instances, and, leaving out all questions of cost, the results are said to be encouraging. The experiments have always been on a very small scale, and in such circumstances it is possible that this method of lighting may be fairly economical, but for regular work on a larger scale the expenses will probably be too great, and it will therefore be necessary to fall back upon purely mechanical means.

Several ingenious plans of driving a dynamo from the axles of one of the carriages of a train have been suggested, but as the power is obtained from the locomotive in the first instance, it would seem to be simpler and cheaper to drive by a separate engine supplied with steam from the main boiler, unless it be thought desirable to turn the lights out whenever a train slackens speed or comes to a standstill before arriving at its destination, in which case the axle contrivance would be perfectly self-acting. That arrangement might sometimes be objected to, and to meet the difficulty the use of secondary batteries has been proposed, but as these batteries are not a success, there is as yet but a small field for the introduction of axle gears.

Another system is that with which Mr. Preece's name is associated; and here the axles of a coach are to drive an air-compressor, delivering its air into receivers, from which a small engine will get its supply whenever the electric light is wanted. Now, if air be compressed isothermally, or if it be allowed to cool after having been compressed adiabatically, it never gives back any of the work expended upon it during compression; and as the amount of intrinsic energy exerted by the air when expanding between such moderate limits as obtain in ordinary practice is small, the efficiency of the compressed-air system must be very low. If the intention is to raise the temperature of the air *after* compression, the result will be different; but in any case the air-engine would never be half so economical as a steam-engine fixed on the locomotive, and there would on the whole be more chances

of a breakdown with the former complicated arrangement than with the latter comparatively simple gear.

After careful consideration, the writer respectfully submits as his opinion, that if the electric lighting of trains is to be made a success in every sense, it must be carried out in the manner which he has indicated, and, judging from what he saw of the recent experiment on the District Railway, there seems every probability of that opinion being confirmed.

The PRESIDENT: There are several very interesting points touched upon by Mr. Massey in his paper, and he has referred to the work or the opinions on the subject expressed by several members, who will no doubt be ready to answer for themselves in the discussion.

In regard to the objection raised by some on account of the brilliancy of electric lighting, I think that in some places, such as the Savoy Theatre, for instance, it is a fault that the electric light is so brilliant. When looked at from the point of view of electric *versus* gas lighting, it is said to be exceedingly successful, but I think that it is often stronger than it ought to be. One point in connection with the consideration of the subject is the question, at what candle-power incandescent lamps should be used so as to be the most economical. We know that if incandescent lamps are worked at a very high candle-power they will not last so long as when worked at a low power, but in the latter case the light will cost considerably more in the production. The proper point, then, to be reached will be such that the extra expense of renewal of the lamps, in consequence of burning them at a considerable candle-power, is just balanced by the extra cost of production of the light at low candle-power; and I think that now we have the means of arriving at some definite conclusion on that point from the facts Mr. Massey has brought before us, and from other tests which have been made. In my presidential address I gave the results of the experimental tests made at the Crystal Palace Electrical Exhibition, which show the amount of energy expended in producing light of different degrees of candle-power from the same incandescent lamps. By comparing

Mr. Massey's figures and facts with the characteristic curves of incandescent lamps, we may arrive at once at that illuminating power which is the most economical, taking the cost of the lamps and the cost of the production of light by them into account. I happen to have the characteristic curves before me, and on referring to them I see that, comparing a Swan lamp at 10 candle-power with the same lamp at 18 candle-power, the cost of production for 10 candles would be just four-fifths of the cost for 18 candles; so that, if we take eighteen 10-candle lamps and ten 18-candle lamps to give the same light of 180 candles, we should have to expend an amount of energy for the 10-candle lamps equal to nearly one and a half times that we should have to expend for the 18-candle lamps, the exact ratio being that of 144 to 100. This result is obtained from the determinations which were made with Swan high-resistance lamps, which are the latest and best which have been produced.

I have no doubt we shall have an interesting discussion on this subject, which has been so well put before us by Mr. Massey.

Mr. W. H. Preece: I am sure, Mr. Chairman, that we all, whether we are interested pecuniarily or not in electric lighting in trains, must feel very much indebted to Mr. Massey for having brought this subject before us; for if in this civilised age there is anything more disgraceful than another, it is the abominable darkness in which we are compelled to be confined during those long railway journeys some of us have to make to the north of Scotland and different parts of this country.

The difficulty that I have always foreseen in lighting trains by electricity is briefly this—that, as a rule, trains are not made up in a solid body such as is the case on the Metropolitan Railway: trains such as those that go to the North along our great northern routes, or to the West by our western trunk lines, are made up in various sections. These sections must be looked upon as portions of a train which is broken up, and we must look upon each portion of a train as a distinct unit. For instance, a train that leaves Euston contains a portion that goes off to Holyhead on the one side, another portion goes to Liverpool, another to Manchester, and another to Glasgow and Edinburgh, and then

on to Aberdeen. When such a train leaves Euston, if it were made up as Mr. Massey has described, we should have each van complete in itself, with its engine, its boiler, its dynamo, and its youth in attendance. It is necessary that each portion should be so supplied, because it very often happens that at a junction, such as Crewe, a train remains perhaps for half or three-quarters of an hour without its engine, and, if the portion be not complete in itself, for that period of waiting it must remain in darkness. The main objection I have always had to the use of steam has been, that there must be a supply direct from the engine, or it must be carried out as stated by Mr. Massey. Now the objections to steam supplied direct from the engine are self-evident: the train must remain in darkness whenever the engine is disconnected. The objection I have to the plan as carried out by Mr. Massey is, that every guard's van must be supplemented by a guard's locomotive, or, in other words, a van with boiler, with steam-engine, with dynamo, and an attendant.

Although the objections to steam are very considerable, they may be got over. All objections, more or less, by experience, are got over, and it may happen that the objections to steam will eventually be got over; but I think there are one or two other modes in which they can be got over, and one is by abolishing the use of steam altogether. The London, Brighton, and South Coast Company have tried to do this by the agency of secondary batteries; and I am bound to say, from all reports I have heard, and from what I have seen, that electric lighting by secondary batteries on the Brighton line has been a decided success. But I only say that they have been a decided success so far as the electric lighting is concerned, for whether the use of secondary batteries has been a success is rather problematical. The arrangements that the Brighton Company had with the Electrical Storage Company were very satisfactory to the Brighton Company, because they were not responsible for those batteries. The Storage Company arranged to renew the batteries as often as they failed; and how often they do fail nobody but the Storage Company knows. The objection that I have to secondary batteries is simply this, that up to the present time it cannot be said that

any single secondary battery, for any purpose whatever, has proved to be a success, although there are very encouraging symptoms that the secondary battery is emerging from the depths of despair into which it has been thrown by financial operations, and is gradually merging into a practical form; but up to the present moment that form for railway train purposes has scarcely been reached. Indeed, I scarcely think it possible, with our knowledge of storage batteries, with the incessant shaking and jolting and turmoil to which the battery and its liquids is subjected in trains, we can increase the efficiency of those instruments; but I cannot speak on this point from experience. Perhaps we may hear something from members present who have had experience.

The next experiment made has been with primary batteries, and some very extensive experiments in this direction have been carried out on the Midland, Great Northern, South-Eastern, and South-Western Railways. I will not say a word about this form of lighting, because I see present two or three gentlemen who can speak from experience; and in these meetings we always rather like to hear the results of experience than the efforts of imagination. In looking at the question broadly, I do not see why a primary battery should not succeed for lighting a train. There can be no such trouble involved in preparing a primary battery as there is in preparing an oil lamp;—oil lamps require cleaning and refreshing at the end of each journey, and so does a primary battery;—and if a primary battery is found which will economically bear the incessant attention it requires, and maintain such lights as Mr. Massey used on the Metropolitan Railway, then I say I am one of those who will hail with great satisfaction the primary battery.

It may be true that when you compress air there is a good deal of energy lost, but I felt that in the motion of a train, as it descends hills and moves about, there must be a considerable amount of waste energy to utilise, which will compensate the other which disappears, and if we can by any means utilise and store this waste of energy, we shall certainly succeed in producing light at the lowest possible cost. For that purpose I

suggested that the axle of the wheels of the van should work pumps, and that these pumps should compress air into a reservoir; that this reservoir into which the air is compressed should work an air-engine, and that this air-engine should directly work a dynamo. By such means a dynamo would be got to move with absolute uniform velocity—a difficulty that is not surmounted when a dynamo is worked direct from the axle of a carriage. By it a store of energy is obtained which can maintain the engine going for any length of time, and I am bound to say that the experiments that have been carried out fully justify the opinion I have formed. Very extensive experiments in this direction have been tried on the London and South-Western Railway, with the result that there is no difficulty in maintaining a reservoir charged up to 500 lbs., 750 lbs., or 1,000 lbs. per square inch, and as a matter of fact it is easier and better in every way to maintain the pressure at the reservoir at 500 lbs. per square inch, and that is done without the least difficulty or trouble. The difficulty which has been surmounted is that to which Mr. Massey has alluded, viz., the production of heat in the compression of the air. It happens that, at the same time that the compression of air involves the production of *heat*, the action of an air-engine results in *cold*. Now, while heat is produced at one side and cold at another, it is readily imaginable that if the two effects be brought together they neutralise each other. This neutrality is produced in the simplest possible manner, by putting the compresser and the engine into the same tank full of water, and then the heat generated on the one side is neutralised in destroying the cold produced on the other.

As regards the cost, I do not think that is worth a moment's consideration. Whether trains be lit by oil, by gas, or by electricity, the item of cost, as compared to the rest of the working expenses of a railway, is a mere flea-bite. As a matter of fact, the cost of lighting trains by gas is cheaper than lighting them by oil; and Mr. Massey has given us figures, which I can fully verify, that the cost of lighting by electricity will be still less than lighting by gas. The cost of fitting coaches (which Mr. Massey has not given us) for electric lighting will also be less

than that for gas. The absolute figures, as far as I have been able to get them, is that the cost of fitting up for gas-lighting is £65 per coach; and if Mr. Massey's, or other figures be taken, it will be found that the cost of fitting up a train for electric lighting is very much less than £65.

Mr. Massey: £170 altogether for 50 lamps.

Mr. W. H. Preece: Well, compare that with gas. The two things are not to be compared for one moment, because £65 per coach means, for about 8 or 10 gas lights, about £6 10s. per gas light, as against about £3 10s. per electric lamp.

The information Mr. Massey gave us about renewals was very interesting, and it fully confirms all our experience in other directions, which is, that if you want your lamps to last, burn them low: if they are 20-candle-power lamps, be satisfied with 18 candle-power; if they are 10-candle lamps, be satisfied with 8 candles. If you do this you will find that the lamps will last almost for ever.

Mr. Gammon: You have heard what Mr. Preece has said about the different systems for lighting railway carriages. I am a railway man, and have taken an interest in the subject from the very first, in the endeavour to ascertain the best system for lighting railway carriages by electricity. Some time ago I was shown a lamp and a very small battery which Messrs. Holmes & Burke said was capable of a great deal, and from my experience their statement was justified. Their system is very simple, consisting of a primary battery. To test it and to see how it behaved when handled and moved about, a battery of nine cells was brought before the directors of the South-Western Railway, at Waterloo Station. Those nine cells supplied a current for six incandescent 5-candle lamps, which were kept going with a brilliant light for something like six hours. The deputy-chairman (Mr. Portal) asked whether the cells and lamps could go as far as Basingstoke and supply light during the journey for two compartments. I replied, "Oh yes; at any rate we will try." The lamps were left burning until about four o'clock; they were then taken to Clapham Junction, fitted on to two compartments, and brought back to Waterloo; we then ran to Basingstoke, and the lamps con-

tinued to burn as brilliantly as at first. In five minutes the cells and lamps were removed from the down train, and in another ten minutes they were fitted up in an old carriage, in which the return journey was made. The battery was placed at the end of the train, but, though we travelled at something like fifty miles an hour, there was not the slightest hitch from oscillation or anything else, and the lights continued to burn magnificently.

The great advantage of these primary batteries is, that by their use it will cost less than half that of other systems to fit up each carriage with its own battery, and so enable the train to be cut up and divided in any way. I recommended the system to the Great Northern Company, and they have employed it successfully since October to light up the Pullman car running to Leeds and back every night. There have been one or two hitches caused, for instance, by the connecting wire coming in contact with the zinc carriage roof, but remedy was speedily effected. The Midland Company have adopted the same system to light their 5.30 p.m. Bradford express from St. Pancras; the South-Eastern Company employ it every night on their 8.5 p.m. Dover mail train; and the South-Western are now running it on what are called their roundabout trains around London, so as to let the public have the benefit of seeing what the light is like. Two lights of 5 candle-power in a compartment give a brilliant and magnificent light, and can be fitted up so to illuminate that a passenger can read with ease and pleasure. It has been successfully tried in Lombard Street for house lighting; the Press took the matter up, and I can only tell you that I am now getting letters from all parts of the world asking whether we can light churches, chapels, or other places; and it is right to say that any man can have the system in his house at a very small cost. The cost has been worked out, not by myself, as I am neither an electrician nor an engineer; and people who are well versed in the matter find that to fit up a house of, say, £60 a year, will cost about half the amount that it costs with gas at 3s. a thousand, with a most brilliant light. Further, when the product of the battery is taken into account,—it will be considerable with large consumers,—the cost will be reduced to almost nothing, and there

will be no special attendant required. I have been looking at a battery to-day which does not even require the top removing, but a funnel is inserted in a hole in the top for charging purposes, which is an operation necessary fortnightly. My experience of primary batteries, as far as it has gone, shows that they are very useful things; whether the Holmes and Burke is the best I do not know, but as the question of primary batteries was mentioned, I have just told you, I think, about all I know of them.

Mr. J. N. Shoolbred: As Mr. Houghton, the Telegraph Superintendent of the London, Brighton, and South Coast Railway, is not present, Sir, perhaps it may be of interest if I briefly mention the salient points of the experiments on the Brighton Railway, and which have been already alluded to. Mr. Houghton explained them to me when I had the pleasure of recently being in one of the trains lighted by electricity. The Brighton Company have for nearly two years been lighting trains by electricity in different ways. They first commenced with secondary batteries, which were charged at the Victoria terminal station, but latterly they have adopted the system, referred to by Mr. Massey in his paper, of a dynamo driven by the axles of the train; and I understand that they feel so satisfied with the result that they have fitted up a second train, and have the intention to fit up succeeding ones. As regards what is to be done, Mr. Houghton would himself be best able to say; but with regard to the train now lighted I can add that 30 lamps of 20 candle-power each are used on that train. The machine employed in that particular case is a Siemens' D2, worked from the axle of the brake-van in which it is situated. Twenty-two Faure-Sellon-Volkmar secondary cells are charged by the dynamo, and they supply current for the 42 volt lamps just mentioned.

In the second train which has been fitted up, a Brush dynamo machine is used; the lamps are of the same calibre, 20 candles and 42 volts, and the secondary cells are reduced to 20. It would be interesting to hear from some one present what has been done in this direction on the Continent, as experiments somewhat similar to those described in Mr. Massey's paper have been carried out some time back, notably in Austria.

Mr. W. E. LANGDON: An experiment with the Holmes-Burke battery is in operation on the Midland Railway. We have a carriage, consisting of six compartments and a guard's van, fitted with the incandescent light. There are two lights in one compartment, one light in each of the other compartments, and two lights in the guard's van. The battery is placed in the van. The wires are carried along the roof of the carriage, and the lamps are fixed in the inside of the roof in the same manner as are the ordinary oil lamps. The van is capable of being uncoupled and coupled as may be required. The lamps are of 5 candle-power, and the illumination of the compartments is very good, and is regarded as a very great advance upon the oil lamps; still, I think improvement can be made. Although you can read very well in the compartment containing two 5-candle-power lamps, it is not so in the remaining compartments, which have only one 5-candle-power lamp.

There is one thing to be said, the fittings of the first-class compartments absorb very much more light than the third-class compartments, and that may account for a better light being obtained in the latter. I believe, however, Mr. Holmes intends to change the lamps, and so produce a more brilliant light. The battery consists of 15 cells, three parts of 5 cells each. The electro-motive force of the battery is 1·85 volts, and its resistance is one-fortieth of an ohm per cell. The lasting power of the battery is 10 hours. The battery now receives daily attention. The experiment is being carried out in connection with the London and Bradford express, and the single journey occupies 5 hours 5 minutes. There is a certain time during which the light is made use of when the train goes through tunnels; the consequence is that the light is not run economically. The liquid which keeps the battery going has to be replaced daily. My experience with regard to the plates, so far as I have been able to ascertain, is that they require re-amalgamating about twice a week. The liquid employed is sulphuric acid (diluted 1 to 12) in the zinc cell, and a patent preparation, which Mr. Holmes has termed oxydone, for the carbon cell. Fumes are given off by the battery, and it is necessary that they be carried away. This is to a certain

extent objectionable; but I am bound to say that in the experiment, so far as it has gone, those fumes have not been found in any way to interfere with the comfort of those whose duties require their presence in the van. The experiment, so far as the Midland Railway is concerned, is regarded, I think, only with the view of testing the applicability of the electric light to railway travelling, and with respect to that I should say that the experiment has been perfectly successful.

The question of cost is one which I do not think it would be be fair to go into upon so small an experiment. It is very evident that the battery is running under exceptional circumstances, and it is not at the present moment being used economically—a great portion of it is being wasted.

I quite agree with Mr. Preece—and for that matter all railway directors and all railway men generally will agree—that there is ample room for improvement in the lighting of railway carriages, and I feel quite sure that every railway manager will be very delighted if a way can be seen to the adoption of a better light: whether it is Pintsch's gas or whether it be electricity, I suppose the question of cost will determine. It appears to me that the question of the cost as regards lighting by a primary battery cannot be dealt with until we have some more complete experiments—that is, larger trains fitted. There is one point which it might perhaps be desirable to draw attention to, and that is whether railway carriages should be provided with one or two lights in each compartment. My impression is that no compartment should be without two lights, so that in the case of anything happening to one, the rupture of its carbon filament or something else, there should be another lamp left to illuminate the compartment. If you use 10- or 20-candle-power lamps, that, again, will alter your arrangements for the source from which you derive your lighting power. Undoubtedly the most complete way of lighting a train would be for each carriage to carry its own source of light, but that is hardly possible, I presume; certainly not, I should say, with regard to the primary battery, for with primary batteries that require to be charged every day it would hardly be possible to take the cells out and replace them to the

necessary extent. Also the question of the quantity of force that has to be provided must be considered, for whether it is a primary battery or whether it is a dynamo, a certain force will have to be provided for a certain number of carriages which will have to be lighted from that source. These are all questions which I should imagine would engage the attention of those gentlemen who are engaged in attempting to introduce the electric light to railway travelling, but they are questions of moment, because there are no railways in the kingdom but whose traffic is subject to be broken up and put together in a very haphazard sort of manner. In some cases you may find as many as eight or ten carriages between the guard's van and engine, and in others perhaps only three or four carriages, and some system of controlling the power will be necessary. In the experiment on the Midland Railway we have a switch which is placed in the guard's van and under the control of the guard, who has the power of increasing or decreasing the battery power as he chooses. I should mention that, although the train has been running for nearly four weeks since the electric light was fitted, we have only experienced one instance of failure of the lamps—the filament of one of the carbons was destroyed.

Mr. Alexander Siemens: Three years ago we made some experiments on the North of Scotland Railway with an electric brake, and for the purpose had a small Brotherhood engine and a D3 Siemens dynamo machine on the locomotive, working the brake. We fitted up about thirty Swan lamps in the brake-van and in some adjoining carriages, to show the effect to the railway company. They liked the light exceedingly, but when we told them at what cost we would fit up the train they declined, and said they had no money. I have not had time to look the matter up, but I know we designed fittings for coupling railway carriages together, and so on, and submitted the arrangement to Mr. Langdon (if I am not very much mistaken), with an offer to fit up a Midland train and run it for a month, if at the end of that time, the arrangement being considered satisfactory, the Company would undertake to pay a certain sum for it, but they had no money. Since that time our attention has been engaged upon

other things, but I find that our machines have been used, notwithstanding, for train-lighting, so we are quite content.

Various difficulties in the way of train-lighting have been referred to. Mr. Massey, for instance, thought it should be first proved properly that the engines and dynamos could run for several hours at a stretch; but I think that everybody who is connected with ship-lighting knows that it frequently happens that the engine-room lights in a ship run from the time that the ship leaves Liverpool until it arrives at New York; and I think that as there is no difficulty in such long runs, so there would be no difficulty on railway trains. Lamp fittings are said to have given some considerable trouble, due to the vibration of the train; but for ship fittings various devices have been invented which overcome any difficulty with vibration; so I do not think there should be any serious difficulties on that score with trains. The main difficulty is that arising from the splitting up of trains. That difficulty must exist in such cases as where the dynamo machine is placed on the locomotive, because no express locomotive will take a train from London to Scotland, but at the first large junction the locomotive is changed, and the train would be in darkness at least for a few minutes,—a thing which of course cannot be allowed,—therefore the arrangement which Mr. Massey adopted on the District Railway would also be necessary for the main lines. A complete apparatus should be placed in the guard's van, and then in all cases where trains have to be split up it would be simply a consideration for the railway managers to say whether any section of a train was important enough to carry its own apparatus or not, and this would not be so difficult as it looks. If a train leaves Euston it hardly ever splits up in more than three parts—one to Liverpool, another to Manchester, and another to the North. Each portion consists of at least six carriages, requiring about sixty lights. Fifty to sixty lights make a very good unit of plant, so that the difficulty would not be so very great.

Also, if some carriages fitted with electric light, and others not fitted, were made up into a train, it is a very easy matter to hrow a piece of cable over the carriage which is not fitted, and

join all the carriages which are fitted with electric lamps together. So I think, if the railway people really made up their minds that they wanted the trains electrically lighted, they could easily have it.

As regards producing the electricity, I think the simplest way is the best; and the simplest way is the steam-engine and dynamo machine. In introducing compressed air or secondary batteries, you are simply introducing a second transmutation of energy, and that is always attended with loss, which cannot be made up with the convenience which different people pretend.

I say nothing about primary batteries, because I think the idea that primary batteries can give economical light has been exploded a good long time ago. With regard to that, I can only say that several times inventors of primary batteries have come to us and presented wonderful results. We have each time offered to take up the battery if the inventor would place it in our hands for a week, explaining exactly of what it consisted and how it ought to be worked, when, if we were satisfied ourselves that the electrical results were as stated, we would take up the battery and pay the inventor any amount of money for it. Up to now nobody has got that amount of money, and I think that is the real proof of the pudding. Of course, it is perfectly easy to make a primary battery work an electric light,—anybody can do that: people did that fifty years ago,—but nobody has yet been able to do it economically; and I think that all experimenting with primary batteries is throwing away money, because the electric light cannot be produced in that way economically.

Mr. R. E. Crompton: I should like to add a few remarks to Mr. Massey's very valuable paper. I happen to have had a good deal of experience in the use of the Willans engine that he used for this experiment, and I think that perhaps Mr. Massey has not dwelt sufficiently on the fact, that a great portion of the praise which must be given to all concerned in the Metropolitan Railway experiment, must be given to Mr. Willans himself, who has shown great ingenuity in putting together the apparatus required for those experiments. His engine, taken by itself, is one of the most beautiful pieces of steam machinery that is in

use at the present day. I do not think that this is too strong an expression to use. His engine is beautiful in an engineering sense, as, although it is very simple in construction and the number of moving parts is exceedingly small, it gives a very high economical result. I believe I may say without fear of contradiction that it is the only high-speed engine that has given results at all approaching the economy of large engines; and as it can run at a sufficiently high speed to be coupled direct to the dynamo, and thus do away with all belting and gearing, it gives us the simplest combination of engine and dynamo that we can have at present. Mr. Massey's figures show that even the very small engine placed on the train used only 30 lbs. of water per indicated horse-power per hour, which is a very high efficiency for so small an engine, and I have every reason to believe that Mr. Willans has obtained even greater economy from his larger engines.

As regards the durability of these engines, I have had one in use driving my saw-mill for three or four years, and it has worked continuously without showing any of the defects common to high-speed engines. The cost of repairs has been very small, in fact rather less than that of ordinary horizontal steam-engines giving the same power for the same time.

Turning to the objections made by various gentlemen, that when a train lighted as described in the paper is divided into several portions the light would be cut off from those portions, the very obvious reply is, that if the apparatus for producing the current be placed on every engine, the only time when the passengers would be deprived of light would be when the detached portions of the train were standing in the station waiting for the fresh engine. This is a defect, but it is an apparently small defect. What if the passengers were left without light for five minutes? At present they sometimes spend the whole night without light; at any rate this inconvenience is a small one when weighed against the great advantages the system presents in simplicity and economy.

I do not like to touch on the question of the use of primary batteries, as it is one calculated to make electrical engineers lose

their tempers; but I think we all agree with Mr. Alexander Siemens that it is absurd to talk of lighting railway carriages with 5-candle-power lamps. The public would not be satisfied with such an extremely small amount of light, which would be little better than the present conditions.

I think Mr. Langdon is correct in saying that there should be two lamps in each compartment; and as we learn from Mr. Massey that two 18-candle-power lamps were used in the experiments under discussion, and that although when passengers first got into the train they thought the light was brilliant, but when accustomed to it they found it insufficient, really points to the fact that two 18-candle-power lamps, or at least two 16-candle-power lamps, will be required for efficient train-lighting.

I need not dwell on the vast difference between the experiment now under discussion, which has proved the practicability of lighting a train with 100 18-candle lamps, and one with a primary battery lighting several compartments each with one 5-candle-power lamp only.

I can go beyond Mr. Massey on one point, for I can testify to the fact that incandescent lamps give out commercially a higher efficiency than they gave in his experiments. I have been making careful tests for some months past, and I have proved to my own satisfaction that Swan lamps can be worked at the full efficiency claimed for them by the manufacturing company,—that is to say, at the rate of $3\frac{1}{2}$ watts per candle,—and that at this rate of efficiency their life will be very long. The life of the lamps entirely depends on the care in manufacturing, and, from the latest experiments which I have made, I believe that this rate of efficiency can be exceeded without diminishing their life.

Mr. Collet: As one not unconnected with secondary batteries, I beg of you, especially Mr. Preece, to listen to me just for a few minutes. Two years ago, under the direction of Mr. J. P. Knight, the General Manager of the London Brighton and South Coast Railway, Mr. Houghton took up the question of railway lighting. He started with the Pullman-car train eighteen months ago; and, if Mr. Preece had taken the trouble to go to London Bridge to make enquiries, he would have been told that the Railway

Company purchased from the Storage Company 70 cells,—they were purchased right out: they were not lent, Mr. Preece, nor guaranteed,—and of those 70 cells 7 only have been changed in the course of eighteen months. Subsequently Mr. Houghton saw the absolute necessity, for successful railway lighting, of an automatic arrangement which should fill accumulators and admit of their being discharged without any attendance other than that which could be given by the ordinary guard of the train. I am happy to say that he succeeded in bringing out an arrangement which, after several experiments with a van attached to the Brighton train, he placed, on the 19th November, on the local line between London Bridge and Victoria—the automatic gearing being fixed on to a Siemens dynamo which worked 22 secondary cells, and from that moment until now that installation has worked without one hitch of any sort or character.

Encouraged by the success of that train, Mr. Knight, the General Manager of the Brighton line, who is well known to many of us as a man of business, determined to try a second train. That second train will be ready in a few days. It is being fitted with a Brush dynamo machine, just to show that the thing can work all round the trade, a matter which concerns some of us.

As regards the cells, upon which I must confess the whole system turns, as I have said, the twenty-two fitted on the 19th November have not required attention of any kind; a second set came in a month later, and they are in as good a condition as when fixed.

Now Mr. Preece for once, I think, made a slip. He said that the jolting of a train affected secondary batteries. I would suggest to him to study the question and endeavour to discover a patent for encouraging the jolting, for it appears that the jolting which the cells have received every day for eighteen months on the Brighton Railway has not broken one of them, but has probably done them good.

Again, Mr. Crompton, who is a man of extreme courage and energy, said that he did not mind being left in the dark for five minutes, providing a Willans engine is put in the brake; but I will tell Mr. Crompton that such periods of darkness would not at

all suit the female portion of travellers on any railway—a train must not be left in darkness for a minute, and therefore I need not discuss that matter. What is wanted is a light that shall be sufficient, not for five minutes, but for twenty-four hours, that the guard can stand and turn on or off with a switch with absolute reliability. To secure perfect train-lighting, the engine and train must be perfectly independent of each other, and any system in which they are dependent upon each other, permit me to say, is a waste of money. In the splitting up of trains the secondary battery is after all the most essential point, because the size of the secondary batteries can be regulated, so that if it is necessary to divide the train into twenty divisions (which of course is a preposterous number, and, as has been said, three is the usual number), an accumulator battery can be placed in each division. It would be fed from one dynamo, and would contain sufficient light, not merely for the stoppages of that journey, but for the journeys during twenty-four hours. The whole question is really answered, and if gentlemen of the ability of those before me will only give that ability to the subject, and will examine this system which has worked so successfully, the next paper we shall hear on the subject will be a conclusive one.

Professor G. FORBES: The discussion this evening strikes me as probably being one of considerable importance, as it is, so far as I am aware, the first time that the subject has really been ventilated in this country. Mr. Massey has not, in his paper, entered as one might have wished into the previous arrangements in the same field; and it is only right just to mention—although I can do little more than mention—the lighting of certain trains at Munich in 1882. It is right to mention them in this discussion, simply to put on record our recollection of that very successful lighting of trains. There was nothing particular in that lighting as compared with the experiment Mr. Massey has described; but a large number of coaches were lighted by incandescent lamps fed by a dynamo which was driven by an engine which was placed along with it in a waggon, being in fact a similar arrangement to that described by Mr. Massey. That was accomplished by the Edison Company, I believe, and those who

were there will remember how successful it was. No one has yet mentioned a case where the steam-engine was put direct upon the boiler of the locomotive. That was done, as many here will remember, by Mr. Shuckert, who placed, if I remember rightly, a small four-cylinder Abrahams engine on one side, and high up on the boiler. That engine worked a Shuckert dynamo which supplied current to the locomotive lamp,—a lamp, by the way, which has not been seen in practical use in this country,—which lit up the path in front of the train for some 200 or 300 yards with great success.

About primary batteries, I am in the fortunate position of having some time ago been allowed that opportunity which Mr. A. Siemens has been so diligently seeking in vain. I had one of those batteries which are said to produce such wonderful results, and was able to submit it to tests. It occurred some time ago, and it would be unfair to go into details, but the result was, as might naturally be expected, that it was not better than many forms of batteries that we knew of long ago.

Mr. C. E. Spagnoletti: It is always a dangerous thing to express an opinion for other people, but I think I shall be quite right in saying that both the public generally and the railway companies would only be too glad if they could see a way for improving the system of lighting railway carriages. The public would no doubt be very glad to be able to read on a long journey, such as Mr. Preece has described, in a comfortable manner, without having to strain their eyes, as is the case with the present lights, or to have recourse to the railway reading-lamp. Railway companies would like to see the last of reading-lamps, which are so destructive to their property, both from grease and the way in which the cloth of the carriages is sometimes torn.

Mr. Massey's paper is a very interesting one, and one that much good may result from the discussion of; but I am afraid he has not hit upon the proper method for the practical lighting of trains. I feel sure that if any one could show the railway companies how they could improve their present system of lighting, at a somewhat reasonable cost, and practically to meet all the requirements of the working of the service, they would readily

adopt such a system. But I am afraid that the plan Mr. Massey has advocated would not at all suit; in fact there is only one advantage that I can see in it, and that is a guard or driver, while the train is running, has the means of lighting up a train when entering a tunnel and putting it out on emerging from it, but that might be done in another way with other systems. The several disadvantages of this system, in my opinion, outweigh this one advantage. I do not for one moment wish it to be understood that I am opposing the introduction of a system for lighting railway carriages by electricity, because my sympathies are quite the other way; but I want to point out a few of the practical difficulties which must be overcome to obtain a perfect system, and I hope in doing so I may save unnecessary labour and expense. In the first place, if you put your dynamo on the engine, you have to take steam from your boiler. Then the whole stock of engines would have to be fitted with dynamos, because, as Mr. A. Siemens stated just now, one engine only travels part of a long journey. I have a few figures here which will show what a serious item this would be. On the Great Western Railway we have 1,577 engines; in England, Wales, Scotland, and Ireland there are 14,000; and to fit up these engines with dynamos and the apparatus necessary would mean a very serious outlay indeed. In addition, it would be necessary to fit up all the carriage-trucks and horse-boxes, because the carriage-trucks and horse-boxes are continually being used, and form a portion of a passenger train. To do this on the Great Western would mean fitting up 885 carriage-trucks and horse-boxes, as well as 6,085 carriages; while in the United Kingdom over 31,000 carriages would have to be fitted, and nearly 12,000 other vehicles, making a grand total of over 57,000 engines, carriages, carriage-trucks, vans, and horse-boxes. Railway companies now work so harmoniously, that one company frequently runs carriages over a part of a second or third company's system; and if the electric lighting of carriages is carried out, it should be on a uniform principle, or these through coaches could not most probably be lighted. Provision has to be made at junctions where trains divide or where engines are changed, so that the train shall not be in darkness for the time,

as also when a vehicle is put on or taken off a train at any station. If slip coaches are to be lighted, they must form part of the system, and then, when a coach is slipped, the lights would go out both in the train and in the slip carriages, unless the means are provided for immediately restoring the electric connections, which would not be easy to do. Again, any accident to the wire or apparatus is liable to affect the whole system and put the light out in every compartment. A certain amount of extra work must necessarily devolve on the driver to look after it. The system that seems to me most applicable, not only for railway, but also for domestic lighting, is the accumulator system. The more one looks into that system, the more satisfactory I think the results are. If lighting by an accumulator system were adopted on any railway, it would simply require four or five centres at which accumulators could be charged, similar to the hot-water system now in use for foot-warmers. They could be put under the carriage seats from outside, by small doors similar to dog-boxes, and connection could be made by means of foot-plates to bring the lamps into circuit, which could be controlled by a small switch outside the carriage and under the control of the guard. By that means an independent system would be given to each carriage or each compartment as required, and as at present. In any system it is desirable that any additional work imposed on the travelling officials of the train should be as little as possible, because as science progresses the labour of railway employés certainly increases. They not only have to couple up the carriages, but have also to attach the automatic brake coupling. By-and-by they will have the communication between passenger and guard, after which may follow the connections for electric lighting, so that the total means a serious amount of work for men to see performed.

Mr. Langdon mentioned, if I understand him rightly, that there was as much waste with the primary battery when out of use as when in use.

Mr. LANGDON: No. I said that the battery was not being economically used, because it was only working six hours out of the ten. I do not think there is much waste with the battery when not in use.

Mr. SPAGNOLETTI: It would be very interesting if we could get the cost of those batteries: I myself have been unsuccessful in obtaining it. Mr. Crompton remarked that it would not matter if passengers were left in darkness for a short time, and instanced that no inconvenience was caused to them by disconnecting the automatic brake from the engine, or by putting on an extra carriage; but that is a very different thing to putting out the light. In the first instance, no inconvenience could possibly be caused to the passengers, as they do not know whether the brake is connected up or not, but they would soon know whether they are left in darkness or not.

The PRESIDENT: I think, gentlemen, that as we have had a good discussion, and as our evening has now almost gone, it will be better to adjourn the discussion, and give Mr. Massey the opportunity of replying at the beginning of the next meeting to the numerous points which have been raised. By doing so a much more satisfactory termination to the discussion can be obtained than by attempting to conclude it to-night. I will ask Mr. Massey to defer his reply until the next meeting.

A ballot took place, at which the following were elected:—

Associates:

Joseph Crookes.
Hardmann Earle.
Edward George Fishenden.
Lazarus Goldenberg.
Albert Harris Howard.
Henry Lea.
Alexander Reid Molison.

Students:

Frank Hugh Bocquet.
George Herbert Bailey.
Arthur Henry Lea.
Henry Carfrae Seddon.
C. J. Thornton.

The meeting then adjourned until March 27th, 1884.

The One Hundred and Thirty-third Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 27th, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The names of new candidates were announced and suspended.

Donations to the Library were announced as having been received from Captain Beresford, R.E., Member, and T. J. Wilmot and H. A. Burrows, Associates.

The meeting accorded a vote of thanks to the donors for their presentations.

The PRESIDENT: I have now to announce to the Society that the Executive Committee of the Universal Health Exhibition propose to have a series of conferences at the Exhibition, similar to the conferences which were held last year, and also some years ago (in 1879); and, the Society being invited to hold a conference at the Health Exhibition on some subject bearing on the object of the Exhibition, the Council have thought good to appoint a Committee to make arrangements with regard to this matter, and to write at once a letter to the Executive Committee to say that they would be prepared to hold such a conference. I suppose that the conference might last for one day, or possibly might go on longer, if there were a considerable number of valuable papers sent in to be discussed; and I invite members who take an interest in this matter to consider it, and make any communications they may wish to make on the subject to the Committee of the Council, who have the arrangements of this matter. Any suggestions or communications sent in to the Secretary will be communicated to the Committee, and I would invite members generally to take part, so that the discussion on the subjects chosen may be a good one. It is thought that one most appropriate subject for consideration would be Electric Lighting in its relation to Health.

Before calling upon Mr. Massey to reply to the discussion on his paper, I have to announce that Mr. Burke is prepared to

exhibit one of his small train-lighting accumulators, with a lamp, after the meeting. We have two papers which are to be brought before us to-night, both of which ought to raise a considerable discussion, and therefore I will, with your permission, at once call upon Mr. Massey to reply to the remarks made at the last meeting.

Mr. MASSEY: The first question raised by yourself, Sir, relates to the brilliancy of the electric light, and, while agreeing with nearly everything you said, I would ask you to remember that a light which suits us exactly to read or work by may be a great deal too bright to be looked at. I tried the effect several times of getting out of a gas-lighted carriage, where it was just possible to read by straining the eyes a little, and then taking a seat in the Putney train, which was said to be overlighted, and I assure you that the restful feeling was most enjoyable. When judging of the effect of a light we should not look at it, but few of us ever think of this. With regard to the prolongation of a lamp's life, *as a matter of convenience*, I am still inclined to think that in many cases the expense of renewals need not balance the cost of producing the light; but this question can only be answered after further experience.

I would like Mr. Preece to note that the arrangement described was not recommended by me, and that I believe the system of driving by a separate engine placed on the locomotive to be the simplest and best. The difficulties foreseen by Mr. Preece, and by others who joined in the discussion, can be got over in a very simple way, viz., by properly lighting all the stations where engines are changed or carriages taken off and put on. And I was surprised to hear so much about the splitting up of trains, while the question of lighting them in a cheap and efficient manner was scarcely considered at all.

I cannot agree with Mr. Preece as to the amount of energy available when trains are running down hill; but I admit that, if the cost of lighting trains is not to be taken into account, the compressed-air arrangement may answer as well as anything else, except that, being more complicated, the machinery would probably break down very frequently. I maintain, however, that it is

of vital importance to know the cost of producing the light; and, in order to make this clear, let us take a 50-light unit, which we know requires about 7 horse-power indicated, or some 24 lbs. of coal per hour, and compare this with the cost of working by compressed air. Assuming a few impossibilities in favour of this system,—first, that the indicated horse-power of the air engine will be equal to what it is theoretically possible to get out of air pressed up to 500 or 600 lbs. per square inch, and that the heat and cold balance or neutralise each other, so as to make the loss as between the work done on the air in the pumps and the work available in the cylinders of the air engine very small; let us assume, secondly, that the efficiency of the stage pump is very high, that there is scarcely any waste of air by leakage, and that the loss in transmitting the necessary power from the locomotive engine cylinders to the pump gear is not excessive,—we shall find that, with a state of things which we can never hope to reach in practice, the total loss is about 90 per cent., or, in other words, we shall require nearly 70 horse-power to do what we did with 7 horse-power when using a separate steam-engine. The coal consumption would be 240 lbs. per hour, which is equivalent to 6 lbs. extra per mile on the coal bill of an express locomotive, thus making it as bad as a goods engine.

Mr. Gammon told us something about primary batteries, but after what Messrs. Preece, Siemens, Crompton, and Professor Forbes said in reply to him, it is not necessary that I should add anything to the statements in my paper.

Mr. Shoolbred mentioned the axle-driving gear used on the Brighton Railway, and I think that, if he calculates the amount of fuel which must be burnt at the locomotive boiler,—for that is where the power comes from to begin with,—he will find it at least twice as great as that required for driving a separate engine. In this kind of gear we cannot take into account the expenses connected with the secondary batteries, of course, because nobody knows what they are.

Mr. Langdon furnished some very interesting particulars of a small experiment made by him on the Midland Railway with primary batteries, and I am glad to hear that he considers one

5 candle-power lamp insufficient for a compartment. I find that, to be only slightly brighter than gas, one 18 candle-power lamp per compartment is required. I quite agree with Mr. Langdon as to the desirability of keeping the light in if possible, but although it might be necessary to fix a double set of lamps in each train, they need not all be kept alight; for, if passengers are to be allowed to turn the lights on or off as they wish, there is no reason why they should not be trusted to switch in a fresh lamp when an old one fails. The breaking up of trains in the hap-hazard way pointed out by Mr. Langdon makes it difficult to decide upon the number of small units recommended by Mr. Preece and others; and all this goes to show the necessity, almost, of working the light for the whole train by means of a separate engine placed on the locomotive, even if such an engine must be somewhat larger than would ordinarily be required, so that it might be equal to the heaviest work that could come on it. In busy times, when two locomotives are often attached to one train, it might not be desirable to couple up the electric light engines together, but it would certainly be convenient to have a second engine as a sort of stand-by.

Mr. Siemens' experience with railway people was not a happy one, but, as all this took place three years ago, when the electric light was not so well understood as it is now, we may hope that the railway companies will not hesitate to adopt the new light when they see that it is the cheapest and best. With due deference to Mr. Siemens, I would point out that train-lighting and ship-lighting are not quite the same thing. On board ship the machinery must run night and day for a week or two at a time, it is true, but special men are told off to watch it constantly; whereas in the case of railway work the machinery must run for several hours at a stretch without any attention at all from the drivers or guards, whose hands are full enough of work already.

Mr. Crompton thought that I did not speak highly enough of the Willans engine, but I think the tests I put it to speak for themselves. In one of the footnotes to my paper, I mention that the same engine had given better results at a trial which I did not attend. As to the possibility of making the electric fittings

on one railway match those on other lines, a similar question was raised in the early days of air-brakes, and it is probable that the difficulty would soon be got over in the way suggested by Mr. Crompton. With regard to the long life of the Swan lamps, I may be wrong, but I think that lamps working at some of Mr. Crompton's places are not always up to full candle-power; and I doubt, therefore, whether this length of life is not due to shortness of current.

Mr. Collet treated us to a sample of the wild talk which people who have a bad attack of secondary-battery fever often indulge in. For Mr. Collet himself I have the greatest respect, and I think that no one can help admiring his courage in a losing race; but with the astute party which seems banded together to impose upon the simple I have no sympathy, because I believe it has done more to prevent legitimate business than all the enemies of electric lighting put together. Why is it waste of time and money to light trains by purely mechanical means until the day when these batteries may be perfect enough to be employed as auxiliaries? Improved cells are promised us "on and after the 1st of January" each year, and I now say, without fear of contradiction from any thoroughly independent person, that these things never work well out of the hands of those who are in some way interested in the sale of them. The Brighton Railway experiment (for which, by-the-bye, Mr. Collet says 70 cells were supplied, and Mr. Shoolbred told us 22 are in use) may be an exception, but it looks uncommonly like the exception which proves the rule, for here the batteries are said to work well in the hands of persons who have invented a contrivance that is of no earthly use without them. There is a future for such batteries, I feel sure, but that future is still before us; and for the present we can go along very well by taking as little notice as possible of the advice offered by those who would have us sit still until their wares are fit to use. I thank Mr. Preece for having administered what I hope will prove a death-blow to the wretched dog-in-the-manger game which has been played so long and with such disastrous results.

Professor Forbes has very kindly called attention to a former

trial of train-lighting at Munich, and he expressed the wish that I should give a history of everything connected with such experiments. I feel that I am not qualified for the task; besides, I must confess that I am more concerned about what is being done, and what we may expect to do, than as to what has been done in years gone by. Perhaps Professor Forbes will make known as widely as possible what he told us about the very latest primary battery, in order to save the poor railway companies from being victimised.

Lastly I come to Mr. Spagnoletti, who gave us many figures, and who ended by recommending an addition to the intolerable nuisance of foot-warmers. By a curious accident he possesses the only secondary battery that would render such a scheme possible, and is it not natural, therefore, that he should see grave difficulties in the way of simple mechanical means, but none in that of the foot-warmer plan? Mr. Spagnoletti told us that there were as many as 1,500 odd locomotives on the Great Western Railway; but supposing he had ten times that number, and a proportionate number of coaches, would he not fit up his trains with the best and cheapest light in the market? He is silent on the question of the cost of lighting trains by means of the foot-warmer battery charged at certain centres, so I cannot compare notes with him; but I am confident that when he goes into the matter he will find my way the most economical. To fit the electric light to every locomotive vehicle on the Great Western Railway would cost about a quarter of a million of money; and to put gas apparatus into 14,000 coaches, etc., would cost, say, £50 per coach (including a proportion of charges for buildings and all fixed plant for gas-making and pumping), or nearly three-quarters of a million of pounds sterling,—three times the price of doing the work in the way I recommend,—and yet Mr. Spagnoletti says that the thing is not practical.

In conclusion, Sir, I own that my views are those of a mechanical engineer who is anxious to see what can be done with the electric light, but I have heard nothing in this discussion which affects the opinion I expressed in my short and very imperfect paper; and I trust that when we have all thought the

matter over a little, we shall see our way to improve what Mr. Preece called a disgrace to the present age. If we hesitate, however, simply because our schemes fall short of absolute perfection, we shall only play into the hands of those who are not too anxious to alter the existing state of things.

The PRESIDENT: I am sure I must congratulate the Society on having had this very satisfactory paper from Mr. Massey, because it led to such an excellent discussion at our last meeting; and now you yourselves have shown your appreciation by the reception you have given to the answers we have had from Mr. Massey to all the numerous points which were brought out in that discussion. We see very well that we might no doubt have spent the whole of this evening in prolonging the discussion, if it were not that other papers are pressing on which also require, and ought certainly to have, a very considerable discussion. We cannot say that the subjects touched upon in Mr. Massey's paper are yet exhausted, or likely to be in one night more, but I am afraid it is impossible for us to give more time to this subject. I therefore propose our hearty thanks to Mr. Massey for his communication.

The following paper was then read:—

ON THE RELATION WHICH OUGHT TO SUBSIST BETWEEN THE STRENGTH OF AN ELECTRIC CURRENT AND THE DIAMETER OF CONDUCTORS, TO PREVENT OVERHEATING.

By Professor GEORGE FORBES.

Sec. 1. *Introductory.—Sec.* 2. *Historical Summary.—Sec.* 3. *Bare Conductors.—Sec.* 4. *Aerial and Subaqueous Cables.—Sec.* 5. *Buried Conductors.—Sec.* 6. *Coils.—Sec.* 7. *Conclusions.*

SEC. 1. INTRODUCTORY.

The heating of conductors by the passage of an electric current is injurious to the insulation if the conductor be insulated, and may lead to risks from fire.

In small installations the heating of conductors is always small, because of this fact—that if contractors were to lay down wires so thin that overheating ensued, then we may be sure that

the resistance would be far too great for the capabilities of the dynamo machine.

But in large installations, currents of much greater density being carried, the heating may be very great although the resistance of the circuit is small; and it becomes a matter of the utmost importance to know how the heating depends upon the size of conductors and the current density.

I have searched in vain for experimental facts on a large scale, and in absence of these have undertaken the mathematical solution of the problem, and confirmed my results by a few experiments on small currents, besides such isolated examples of measurements of large currents as were available.

I have been at some trouble to determine carefully the nature of conductors which would be required to carry a current capable of supplying 100,000 lamps—say, 70,000 ampères. It may be said that no such conductor would be required—that electricity will be so carried in a network of conductors that in no part will the current carried be excessive. It may further be said that high tension currents will be used to charge accumulators in series, scattered here and there over a district, and that, consequently, small currents only will be required in the mains. To the latter objection, I say that, to carry out some of the provisional orders granted by the Board of Trade, the system of secondary batteries being inadmissible, it will be necessary to carry through the mains, current sufficient for all the lamps. To the former objection, I say that the supply of gas gives us a valuable insight into the similar progress which must be made in the supply of electricity. The problems are remarkably similar, and a due attention to this fact will save the pioneers of electricity much useless expenditure of time, money, and thought. But in gas lighting we carry enormous mains for distances of many miles from the place of manufacture. Witness the huge 4-feet pipes laid through this district last year, to carry gas from Wandsworth to the City. There is no network of conductors here: it is found necessary to carry in one main, gas enough to supply hundreds of thousands of gas lamps. Let it be well noticed, also, that it would be possible to force the gas at high pressure through narrow tubes to fill and supply gas-holders spread about in different

parts of a district. The analogy to the proposed system of charging accumulators at high tension is perfect, and this leads me to doubt very much whether the system which has not been found advisable with gas is likely to be successful with electricity.

I still maintain that, to supply the electric light on a large scale, we must face the problem of finding out what conductor will carry a current of 70,000 ampères without overheating.

Now, in doing this we are going a step in advance of what has been done before, just as (to cite, as example, a contemporary engineering work), in designing the Forth Bridge, Messrs. Fowler and Baker are extending the principles of bridge-making to magnitudes hitherto unknown. Here the laws of the stability of bridges are known, and, with experience on smaller bridges, combined with laboratory tests of the strength of materials, a sure advance can be made to the larger structure. It has been my endeavour to find out whether, with the facts at our disposal as to the smaller currents carried by smaller conductors, and the laboratory experiments on the nature of our conductors and insulators, we are in a position to propound laws which shall be a guide to us in extending these principles to the construction of a suitable conductor for very large currents, say, of 70,000 ampères.

SEC. 2. HISTORICAL SUMMARY.

In 1882 the Fire Risks Committee of this Society discussed the question, and I believe I am right when I state that it was seriously proposed as a rule, to prevent overheating of the wires, that the permissible current should be so many ampères per square inch section. I have often heard this error repeated. I believe it has actually been adopted by the fire insurance companies as a measure of safety, and a precise 1,000 ampères per square inch has been given as the safe current. With regard to the insurance companies, little harm has been done by this, because they have had only small installations to deal with at present, and, as above stated, there is practically no danger from this cause; but it seems surprising that in one breath they should tell contractors that in small installations they must not raise the temperature of their conductors $\frac{1}{10}$ of a degree, and that in large installations they may make their conductors red-hot.

As a matter of fact, in any installations, except very large ones, the safe conductor ensures greater economy than the unsafe one; and Sir William Thomson has done well* in fixing the size of conductors by commercial considerations, when he showed that the interest and depreciation on the cost of conductors should equal the annual loss of horse-power in heating up these conductors.

There is a limit above which this rule does not apply, because the heating becomes so great that the insulation is injured. The first person who, so far as I know, has taken notice of this, is Mr. Cowling Welsch, in a table published by Messrs. E. & F. Spon. He fixes the limit at 2,700 ampères, but he does not state what he considers to be the limiting temperature which is tolerable, nor does he specify the nature of the insulation. His facts seem to be taken from the tests of Messrs. Clark, Forde, and Taylor (see next page.) Mr. T. Gray has also taken notice of the failure of Sir William Thomson's law for high currents, in a paper contributed to the *Philosophical Magazine* in 1883, and fixes the limiting value at 5,000 ampères.

I shall not take up the question of how Sir William Thomson's rule is to be applied commercially. I have resolved in this communication to confine myself to one point—the strength of current which can be carried through a wire under different conditions without overheating.

I find that two writers have worked at this subject from a mathematical point of view, and each has worked out some concrete examples. One of these is Mr. Day, of King's College, whose useful little book, "Electric Light Arithmetic," should be studied by all learners. The other author is Mr. T. Gray. His remarks on the subject appear in the *Philosophical Magazine*, September, 1883. In discussing the question of bare wires, both of these authors assume that the cooling effect is proportional to the surface, and they make no reference to the variation from this law which I pointed out in 1882 to the British Association, and which has been confirmed by Mr. Preece. Mr. Day deals only with the

* B. A. Reports, 1881, pp. 518 and 526.

case of a naked wire, in which he arrives at the theoretical law—(current)$^2 \propto$ (diameter)^3—which I published in 1882, but which I also showed at that time to be contradicted by experiments on a small scale. My own view of the matter is, that while, of course, radiation is proportional to surface, convection is not so, but is nearly constant for rectilinear wires of different diameters but the same temperatures, and that with thin wires consequently convection is the most important factor, but for thick wires radiation proportional to surface is the ruling factor; hence the tables which I have computed are correct for large diameters, but with small wires greater currents may be safely carried.

Mr. Gray has also gone partially into the theory of an insulated cable, and arrived at formulæ very similar to my own.

Some experiments were made by Messrs. Clarke, Forde, and Taylor for the Indian and Oriental Electrical Storage and Works Company, in the last year or two, and the results are published in the *Electrician* for April, 1883. The general conclusion arrived at is, that up to 10 ampères it is safe to allow 1 ampère per 10 pounds of copper per mile, either with naked wire or with insulated wires buried in sand, in the hot Indian climate. They furnish the following table :—

CLARKE, FORDE, AND TAYLOR'S TABLE.

B.W.G.	Diam. Mills.	Weight in lbs. per mile.	Ampères.	Lbs. per ampère.
22	28	12·4	2·33	5·32
21	32	16·2	2·84	5·70
20	35	19·5	3·27	6·00
19	42	28·0	4·3	6·5
18	49	38·1	5·4	7·0
17	58	53·3	6·9	7·7
16	65	67·1	8·3	8·1
15	72	82·5	9·6	8·6
14	83	109·5	11·9	9·1
13	95	143·0	14·56	10·0

No information is given as to the temperature which is considered permissible.

In the second supplement of the *Electrician*, published in March, 1883, a table was printed, supposed to give the currents which could be safely worked through different thicknesses of conductor. This table, however, was founded upon the assumption that the safe-working current was proportional to the sectional area, which is now well known to be far from the case. I quote this simply as one example, out of many which has come to my notice, of the same mistake being made by people who ought to be better informed.

There are five primary cases of conductors which must be treated separately—

(1) Overhead naked wires.
(2) Overhead cables.
(3) Subaqueous cables.
(4) Subterranean and embedded cables.
(5) Coils.

I have added the fifth case, of coils, because it is important in the manufacture of dynamos and magnets.

Each of these classes has its own peculiarities. In all of them heat is generated by the current, and this heat must be got rid of. In case (1) it is got rid of solely by radiation and convection; in the others partly by conduction, and in case (4) very largely by absorption. In case (1) the maximum temperature is reached almost immediately: in some of the other cases it may be many hours before the final steady flow of heat sets in.

SEC. 3. BARE COPPER WIRES.

Having convinced myself that the most satisfactory mode of attacking the problem was to treat it in a strict mathematical way, and being well aware that all the requisite data had been obtained by previous experimenters, I determined to work out practicable tables for the use of electricians from these data, including both bare and insulated conductors. The first step was to solve the following problem :—

Problem I.—*To find the law connecting diameter*, D, *of conductor with that strength of current*, C, *which raises its*

temperature by a fixed amount t° cent. above that of the surrounding air.

Let R = the resistance in ohms of a cubic centimètre of the substance of the conductor (= its specific resistance).

Let E = the heat radiated per second from a square centimètre surface when the temperature of the surface is 1° cent. above that of the surrounding air.

The radiation from the surface of 1 cm. length of the wire is $\pi\, D\, t\, E$, and this must equal the heat generated in 1 cm. length of the substance $= C^2 \cdot \dfrac{R}{\pi\left(\dfrac{D}{2}\right)^2} \times$ (number of units of heat in 1 joule) $= C^2 \cdot \dfrac{4\,R}{\pi\, D^2} \times \cdot 24$.*

Whence $$\pi\, D\, t\, E = C^2\, \frac{4\,R \times \cdot 24}{\pi\, D^2}$$

and $$C^2 = D^3\, t \cdot \frac{\pi^2\, E}{R \times 4 \times \cdot 24} \quad \ldots \quad \ldots \quad \ldots \quad (A)$$

This shows that if the heat be lost by radiation, or by any means which is proportional to the surface, then, in order to keep all the wires of different diameters at the same temperature, we must have the cubes of these diameters proportional to the squares of the currents if the change of resistance with temperature be neglected.

Example:—To take, as a special example, the case of copper we know that

$$R = \cdot 000001642 \text{ ohm† at } 0^\circ \text{ centigrade,}$$

and increases ·38 per cent. per degree centigrade;

$$E = \cdot 000168 \text{ (polished), or } \cdot 000300 \text{ (blackened).‡}$$

To take an example, let C = 10 ampères; let the wire be

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* Everett's "Units and Physical Constants." Joule's equivalent of a Gramme centigrade heat unit = $4{\cdot}2 \times 10^7$ ergs, and one joule = 10^7 ergs, ∴ ·24 = number of heat units in one joule.

† Maxwell's "Electricity and Magnetism," Vol. I., last chapter.

‡ This is taken from D. McFarlane's experiments (*Proceedings Royal Society, Edinburgh*, 1872, p. 93), in which radiation took place from balls of considerable size, and, consequently, convection played an unimportant part. If the rise in temperature were 100° or more, it would become necessary to take account of McFarlane's second and even third terms, depending on t^2 and t^3.

No. 16 B.W.G. = 0·165 cm.; the rise of temperature comes out

$t = 21{\cdot}2°$ C., polished,

or

$t = 15{\cdot}0°$ C., blackened.

TABLE I.

Bare Copper Wires.—Current required to increase the temperature of a copper wire t° centigrade above the surrounding air, the copper wire being bright polished or blackened.

Diameter in centimètres and mills. (thousandths of an inch).		CURRENT IN AMPÈRES.									
		$t = 1°$ C.		$t = 9°$ C.		$t = 25°$ C.		$t = 49°$ C.		$t = 81°$ C.	
Cm.	Mills.	Bright.	Black.	Bright.	Black.	Bright.	Black.	Bright.	Black.	Bright.	Black.
·1	40	1·0	1·4	3·0	4·1	4·8	6·6	6·5	8·9	7·9	11·0
·2	80	2·8	3·9	8·3	11·5	13·5	18·7	18·3	25·3	22·4	31·0
·3	120	5·2	7·2	15·3	21·2	24·9	34·4	33·5	46·4	41·2	57·0
·4	160	8·0	11·0	23·6	32·7	38·3	53·0	51·7	71·5	63·4	87·8
·5	200	11·1	15·4	33·0	45·7	53·5	74·1	72·2	99·9	88·6	123
·6	240	14·6	20·3	43·4	60·0	70·3	97·4	94·9	131	116	161
·7	280	18·5	25·6	54·6	75·6	88·7	123	119	165	147	203
·8	310	22·6	31·2	66·7	92·4	108	150	146	202	179	248
·9	350	26·9	37·3	79·6	110	129	179	174	241	214	296
1·0	390	31·5	43·6	93·3	129	151	210	204	283	251	347
2·0	790	89·2	123	264	365	428	593	577	799	709	981
3·0	1180	164	227	485	671	787	1090	1061	1468	1303	1805
4·0	1570	252	349	746	1035	1211	1675	1633	2260	2006	2776
5·0	1970	353	488	1043	1444	1692	2343	2283	3160	2802	3880
6·0	2360	463	642	1371	1898	2225	3080	3000	4154	3685	5100
7·0	2760	584	808	1728	2392	2803	3882	3781	5233	4642	6426
8·0	3150	714	988	2110	2922	3422	4741	4620	6396	5671	7850
9·0	3540	851	1178	2519	3486	4088	5659	5511	7630	6769	9370
10·0	3940	997	1380	2950	4084	4788	6626	6455	8935	7926	10973
34·4	...	...	...	...	...	...	...	...	...	...	70000

This table is calculated from the formula

$$C^2 = D^3 t \cdot \frac{\pi^2 E}{4 R \times 0{\cdot}24}$$

C = current (ampères).
D = diameter of wire (centimètres).
t = excess of temperature (centigrade) above air.
E = coefficient of radiation and convection.
R = specific electrical resistance (ohms).
·24 = number of gramme-centigrade heat units in a watt.
Temperature of the air assumed 20° C.

$$R = 0{\cdot}000001642 \left(1 + \frac{{\cdot}38\,t}{100}\right).$$

E = ·000168 for polished, ·00032 for blackened, copper.

The accompanying Table I. has been computed from the formula obtained above. It gives the rise in temperature in bare copper wires with different currents. In computing with this formula, it must be noticed that the value of R, the specific resistance, varies with the temperature. The resistance at 0° C. is 1642, as stated above. At the temperature of the air (which may be taken at 20° C.) it is 1736, and at any temperature which is t° above 20° C. the resistance is 1642 (1 + ·0038 (t+20).) This change of resistance produces a change of 15 per cent. in the current which can be carried at the higher temperatures.

The effects of temperature in altering the resistance are continually cropping up in our application of theory to practice, and the following very striking experiment is worth recording:—

I have been informed by Mr. H. Edmunds that he made experiments with wires $\frac{1}{32}$ inch diameter, flattened out to various widths, through which he passed the current from a machine, the E.M.F. being the same in all the experiments. In the form of wire $\frac{1}{32}$ inch diameter, it was heated to a bright colour; when flattened to $\frac{1}{16}$ and $\frac{1}{8}$ inch width, it lost luminosity; and so on until, when used in a strip $\frac{1}{2}$ inch wide, it kept pretty cool, and fairly stopped the engine. Here we see that the resistance of the wire and all the strips was the same at any constant temperature, but the surface for cooling by radiation and convection was greater with the wider strips. This explains why the wider strips were cooler than the narrower ones, and still more than the wire. Lastly, the resistance is trebled at a temperature which makes the metal barely luminous, and is enormously increased at a bright heat. Hence, in the cases

where there was bright luminosity, there was high resistance and less current. The wide strip, being the coolest, had most current, and used up most work, and so stopped the engine.

In the above table (as in the others which follow it), the current specified heats the wire to the degree stated only when steadily applied. A much more powerful current might be used for a very short time at intervals, as in signalling for railways.

The only doubt of the accuracy of the table can come from a doubt as to the accuracy of McFarlane's experiments, which were made in Sir William Thomson's laboratory, or in the extension of his results to surfaces of different dimensions. On this matter I have a few remarks to make.

1. The value which McFarlane found for the loss of heat per second per degree difference of temperature between the metal and the enclosure increased from $t = 5°$ C. to $t = 60°$ C. in the ratio 178 : 226 for polished copper. I have used the value 178 in calculating the above table, so that the current which can be carried with the copper in any state of oxidation, or dirt, is certain to lie between the two values given in the table under *bright* and *blackened*.

2. The only experiments with which I can compare Mr. McFarlane's are those by the late Mr. Nichol, published by Professor Tait in the *Proceedings Royal Society, Edinburgh*, 1869-70, p. 207. The table on next page gives the comparison.

A comparison of the results of McFarlane and Nichol shows that they agree generally as well as could possibly be expected, so far as the term which depends on the first power of the temperature in the expression

$$\text{loss of heat} = A\,t + B\,t^2 + C\,t^3 + \cdots$$

is concerned, but that in the comparatively unimportant second term McFarlane makes B negative, and Nichol makes it sometimes positive and sometimes negative. The general conclusion is that we can trust safely to the first term, but that we must not push the application to extremely high temperatures.

3. Both the above sets of experiments were made upon masses of metal some centimètres in diameter, and the conclusion seems warrantable that with such masses my formula is accurate.

I state this now, because I have next to show that the law does not extend to small masses where convection plays a more important part than radiation. My impression is that thin wires lose their heat chiefly by convection when free in the air, but larger masses chiefly by radiation.

Loss of heat (per sq. cm. per second per degree centigrade difference of temperature) from copper in air at atmospheric pressure in blackened enclosure at constant temperature (8° *C. in Nichol's experiments*), *for various differences of temperature*:—

Polished.			Blackened.		
Difference of temperature.	Loss per sq. cm. per sec. per degree.		Difference of temperature.	Loss per sq. cm. per sec. per degree.	
	McFarlane.	Nichol.		McFarlane.	Nichol.
Degrees. 10·0	·000176	...	Degrees. 10·0	·000266	...
12·5	...	·000198	12·5	...	·000364
15·0	·000193	...	19·3	...	·000331
15·3	...	·000182	20·0	·000289	...
20·0	·000201	...	30·0	·000306	...
21·6	...	·000175	33·6	...	·000320
30·0	·000212	...	40·0	·000319	...
32·5	...	·000173	42·2	...	·000322
40·0	·000220	...	50·0	·000326	...
42·5	...	·000173	53·2	...	·000328
50·0	·000225	...	60·0	·000328	...
55·8	...	·000177			
60·0	·000226	...			

I worked at the subject experimentally in 1881 and the following years. My results were published in the *British Association Reports*, 1882; *Annales de l'Electricité*, 15th October, 1882; the *Electrician*, 1882, September, and 1883, February.

My first object in those experiments was to test the correctness of the following considerations:—When a current passes through a wire keeping up a constant temperature, the heat developed by the current over a given length is equal to that lost by radiation, convection, and conduction. It seemed right to suppose that at a fixed temperature this cooling varies as the

surface, *i.e.*, as the diameter of the wire. The heat generated by the law of Joule varies as $C^2 R$ or $\frac{C^2}{D^2}$, where C = the current, R the resistance, and D the diameter of the wire.

Whence $$\frac{C^2}{D^2} = a\,D \text{ (} a \text{ being a constant),}$$

and $$C = a\,D^{\frac{3}{2}}.$$

To verify the exactness of this law, I experimented on several wires of different diameters but the same conductivity. Each wire was thinly coated with beeswax, whose melting point was 58° C., the temperature of the room being 18° C. A current was passed through one of these wires, and resistances were slowly and gradually removed from the circuit, until the current heated the wire so as to melt the wax. The angle of deflection of the tangent galvanometer was then read off, to give the intensity of the current. The same operation was repeated on the other wires, and the following table gives the results obtained:—

D	C	$\frac{C}{D}$	$\frac{C}{D^{\frac{3}{2}}}$	$\frac{C}{D^2}$
Mm. 0·58	0·984	1·696	2·229	2·924
1·22	2·304	1·888	1·709	1·548
1·58	3·026	1·915	1·523	1·212

If $C \propto D^{\frac{3}{2}}$, the quotient $\frac{C}{D^{\frac{3}{2}}}$ should be constant for all the wires. If, as some have supposed, $C \propto D^2$, the quotient $\frac{C}{D^2}$ should be constant. If, lastly, $\frac{C}{D}$ is more nearly constant, as is seen to be the case, the law is that the current varies more nearly as the diameter.

Within the last few days I have come across some tests which I had made in 1881, on five thicknesses of lead wire, to find the current required to fuse them. I found that this depended upon the length of the specimen. The reason is that the ends of the wire are clamped by cold metal, which absorbs the heat, and so a

greater current is carried without fusion with short specimens than with long ones. I give the results for what they are worth.

Fusing Currents for Lead Wires.

Diameter.	Length.	Fusing Current.
Mm.	Mètre.	ampères.
0·55	0·025	0·78
0·78	0·025	0·937
0·94	0·025	1·125
1·03	0·025	8·2
1·03	0·225	6·0
1·28	0·300	9·5
1·28	0·150	12·37
1·28	0·075	12·75
1·28	0·050	13·5
1·28	0·025	16·87

These measurements were not made by myself, and I cannot vouch for any very great accuracy. One fact which we learn from them is, that in such experiments, with wires about 1 millimètre thick, the length in experiments of this nature should be not less than 30 centimètres, or, generally, the length should be 300 times the diameter. The effect of using short wires is especially shown with the thicker ones, the experiments on which show that a large quantity of heat is carried off by thermal conduction to the massive cooling terminals.

Taking the case of a long wire, let us see how far it gives us reason to believe in the applicability of the formulæ of this memoir to practical cases. A lead wire, 1·28 millimètre diameter and of considerable length, was heated to the temperature of fusion with a current of 9·5 ampères, and one of 1·03 millimètre diameter, with a current of 6·0: let us find the theoretical current required.

By referring to Problem I., we see that the heat generated per second in one centimètre length of the substance $= C^2 \dfrac{R}{\pi \left(\frac{D}{2}\right)^2} \times \cdot 24$

where C = current in ampères,

R = specific resistance in ohms,

D = diameter.

Now R* $= 19{,}850$ at 0° C. for lead in C.G.S. units.
$= 44{,}751$ at 327° in C.G.S. units,
$= \cdot 000044751$ in ohms.

The melting temperature of lead being 327° C.†, or, say, 310° C. above the surrounding air,

$$\therefore \text{heat generated} = C^2 \frac{\cdot 000044751 \times 4}{\pi D^2} \times \cdot 24 = \cdot 0000570 \frac{C^2}{D^2} \times \cdot 24.$$

Referring to McFarlane's experiment, I find that 60° C. excess of temperature gives a loss of heat per square centimètre per second $= \cdot 01356$ gramme centigrade heat units with polished copper, and that the loss is nearly proportional to the temperature. This would give $\cdot 07006$ for 310° C.

The surface of one centimètre length of the first wire is

$$\pi \times \cdot 128 = \cdot 402,$$

and of the second it is

$$\pi \times \cdot 103 = \cdot 324;$$

and the loss of heat is in the first wire

$$\cdot 07 \times \cdot 402 = \cdot 02814,$$

in the second

$$\cdot 07 \times \cdot 324 = \cdot 02268,$$

and this must equal the heat generated as given above, viz.—

$$= \cdot 0000570 \times \cdot 24 \frac{C^2}{D^2}$$

$$= \cdot 00001368 \frac{C^2}{D^2}$$

$$= \cdot 000848\, C^2 \text{ for the first wire,}$$

$$\text{and } = \cdot 001290\, C^2 \text{ for the second wire;}$$

whence for the first wire

$$C^2 = \frac{\cdot 02814}{\cdot 000848}$$

and for the second

$$C^2 = \frac{\cdot 02268}{\cdot 001290}$$

which gives us 5·8 and 4·2 ampères theoretically in place of 9·5 and 6·0 respectively, as found by experiment. This only shows

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* Jenkin: Cantor Lectures.

† Balfour Stewart: "Elementary Treatise on Heat," p. 88.

that McFarlane's constant does not apply to high temperatures, and that the loss of heat is then much greater than in direct proportion to the temperature.

The only extensive experiments on the subject, with which I am acquainted, have been made by Mr. W. H. Preece, and the results are about to be communicated to the Royal Society. He has been kind enough to show me his experimental results, in order that I might be able to bring before you a comparison with my own results.

He measured the current which was just sufficient to melt platinum wires of different sizes, and he also measured the current which is just sufficient to make wires luminous. The results obtained by Mr. Preece confirm my experiments, and show that with small wires the $(\text{current})^2$ is more nearly proportional to the $(\text{diameter})^2$ than to the $(\text{diameter.})^3$

Sec. 4. Aerial and Subaqueous Cables.

We now come to the case of insulated conductors. There are two cases which can be taken together—aerial and subaqueous. The mathematical treatment of these is, however, not quite the same. In the subaqueous cable we may assume that the outside of the insulator remains at the temperature of the water. In an aerial line it sometimes happens that the insulator is so thin that its outside becomes quite hot. The mathematical view of this case is nearly the same as that of a copper conductor covered with lampblack, which case has already been treated.

Problem II.—*A conductor of radius r_1 is surrounded with an insulator to an outer radius r_2. If the ratio $\frac{r_2}{r_1}$ remains constant, it is required to find the way in which the current* C *must vary with radius r_1, so that the temperature of the wire shall be t_1 degrees cent. above that of the outside of the insulator.*

Let R, as before, be the specific electrical resistance of the conductor in ohms, and let K be the thermal conductivity of the insulator.

Let H be the heat which is generated per second by the current in a length of one centimètre of the conductor.

Then H is also the heat which flows per second radially out of the insulator per centimètre of length. Imagine the insulator to be made up of a number of concentric cylinders, and let the radius of one of them be r and the thickness δr, then the surface of one centimètre length of this cylindrical shell is $2\pi r$; and if $-\delta t$ be the difference of temperature, we have, from Fourier's definition of conductivity,

$$H = -K \cdot \frac{2\pi r \cdot \delta t}{\delta r}$$

If we integrate this between the limits $r = r_1$ and $r = r_2$, the difference of temperatures at these points being t_1, we find that

$$\log_e \frac{r_2}{r_1} = \frac{2\pi K}{H} t_1$$

Now we also know, from Joule's law, that the heat generated in one centimètre length of the conductor is

$$H = \frac{4 C^2 R}{\pi D_1^2} \times \text{(number of heat units in one joule} = 0{\cdot}24).$$

$$\therefore \log_e \frac{r_2}{r_1} = \frac{\pi^2 K D_1^2}{{\cdot}48\, C^2 R} t_1$$

$$C = \sqrt{\frac{\pi^2 D_1^2 \cdot K t_1}{{\cdot}48\, R \log_e \frac{D_2}{D_1}}} \quad \dots \quad \dots \quad \dots \quad (1)$$

It appears from this, that when the ratio $\frac{D_2}{D_1}$ is constant, the current must vary as the radius of the conductor to produce a constant difference of temperature between the inside and outside of the insulator. But it would be comparatively useless to tabulate the data from this formula, for with aerial cables we must take note of the excess of temperature of the outside of the insulator over the surrounding air. Call this excess t_2. Then, from the method pursued in the investigation for bare wire, E being, as before, the coefficient of radiation and convection, the flow of heat is

$$= \pi D_2 t_2 E$$

but it is also

$$= \frac{2\pi K t_1}{\log_e \frac{D_2}{D_1}}$$

whence

$$\frac{t_1}{t_2} = \frac{D_2 E \cdot \log_{\cdot e} \frac{D_2}{D_1}}{2 K}$$

putting E = ·0003 (see above) and K = ·0005

$$\frac{t_1}{t_2} = \frac{3}{10} D_2 \log_{\cdot e} \frac{D_2}{D_1} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

$$\therefore t = t_1 + t_2 = t_1 \cdot \frac{10 + 3 D_2 \log_{\cdot e} \frac{D_2}{D_1}}{3 D_2 \log_{\cdot e} \frac{D_2}{D_1}} \quad \ldots \quad (3)$$

and from (1)

$$C = \sqrt{\left\{ \frac{\pi^2 K D_1^2}{\cdot 48 R} t \times \frac{3 D_2}{10 + 3 D_2 \log_{\cdot e} \frac{D_2}{D_1}} \right\}}$$

This formula is one of great interest. From it we can calculate directly the value of the current or the rise in temperature, when the other quantities are fixed; and all the problems in connection with such cables as are discussed in this memoir can be dealt with by the help of the same formula. There is another matter of great practical importance which it enables us to solve. We can compare it with the formula (A) on page 238, for bare copper wire. Call I and I′ the currents in bare and insulated wires, which with the same value of D give also the same value of t.

Assume E = ·0003 for insulation, and E′ = ·0002 for copper.

$$\frac{I^2}{I'^2} = \frac{D^3 t \cdot \frac{\pi^2 E'}{R \times 4 \times \cdot 24}}{\frac{D_1^2 D_2 \pi^2 K E \cdot t}{2 \times \cdot 24 \times R} \cdot \frac{1}{2 K + E \cdot D_2 \log_{\cdot e} \frac{D_2}{D_1}}}$$

$$= \tfrac{2}{3} \times \frac{D^3}{D_1^2 D_2} \cdot \frac{1}{2 K} \cdot \left(2 K + D_2 E \log_{\cdot e} \frac{D_2}{D_1}\right)$$

$$= \tfrac{2}{3} \cdot \frac{D^3}{D_1^2 D_2} \cdot \tfrac{1}{2} \cdot \left(2 + \left\{\frac{E}{K} = \tfrac{3}{5}\right\} \cdot D_2 \log_{\cdot e} \frac{D_2}{D_1}\right)$$

and $D = D_1$.

Thus we find that I is greater or less than I′, according as

$$2 D_1 \left(2 + \tfrac{3}{5} D_2 \log_{\cdot e} \frac{D_2}{D_1}\right) \text{ is } \gtrless 6 D_2.$$

Take as special cases (1) $D_2 = 2\,D_1$ and (2) $D_2 = 4\,D_1$. Then I is $\gtrless$ I′, according as

$$(1)\ 4 + \tfrac{6}{5}\,D_2 \times \cdot 693 \text{ is } \gtrless 6 \times 2,$$

$$\text{and } (2)\ 4 + \tfrac{6}{5}\,D_2 \times 1\cdot 386 \text{ is } \gtrless 12 \times 2,$$

i.e., according as

$$(1)\ D_2 \text{ is } \gtrless \frac{6 \times 2 \times 5 - 20}{6 \times \cdot 693} \gtrless 9\cdot 6,$$

$$\text{and } (2)\ D_2 \text{ is } \gtrless \frac{12 \times 2 \times 5 - 20}{6 \times 1\cdot 386} \gtrless 12\cdot 0,$$

or (1) as D_1 is $\gtrless$ 4·8 centimètres,
and (2) as D_1 is $\gtrless$ 3·0 centimètres.

If different values of E and K are adopted, these values will vary proportionally to $\frac{K}{E}$, here assumed to be $\frac{3}{5}$.

We have now arrived at a most important result, viz., that an insulated wire carries a greater current without overheating than a bare wire, if the diameter be not very great. Assuming the diameter of the cable to be twice that of the conductor, a greater current can be carried in insulated cables than in bare wires up to 4·8 centimètres diameter of conductor. But if the insulated cable have a diameter four times that of the conductor, this is the case up to 3·0 centimètres diameter of conductor.

When the thickness of insulation is made very great, the limiting size of conductor which favours the insulated wire is shown below :—

$\frac{\text{Diameter of insulator.}}{\text{Diameter of conductor.}}$		Limiting diameter of conductor which favours insulation.
2	......	4·8 cm.
4	......	3·0 „
6	......	2·5 „
8	......	2·2 „
10	......	2·0 „
100	......	1·0 „

I venture to express the conviction that these results must be looked upon as very surprising. It was hardly to be expected that, by surrounding a copper wire with a bad conductor of heat, we could in any case increase the strength of

current which it will carry without overheating. Yet such is clearly the case; and the general explanation of it is that by so doing we increase the surface from which radiation and convection take place. When, however, we have to deal with large currents in large conductors, and the thickness of insulating material is increased in the same ratio, the heat finds greater difficulty in penetrating so thick a mass, and the insulation becomes objectionable from its bad heat-conducting properties, so as to lead us to the result that the bare wire carries more current than the insulated one without overheating, when the diameter is great.

It has been supposed by some persons that heat will escape far more freely through insulating materials, owing to their sometimes being diathermanous, and allowing heat to be radiated through them. Now, in opposition to this view, I must say that very few such substances are diathermanous, and very seldom are they sufficiently homogeneous to allow the possibility of direct radiation through their mass.

Before I go on with what I have to say, I must now pause to make a few remarks on the problem which has just been solved.

1. *Permanent State.*—The definition of Fourier, which has been made the basis of the calculations, has reference to the case only when heat has been steadily supplied for some time, so that the gradation of temperatures from the hot interior and the cool exterior has reached what is called the permanent state. The time which is required to attain this state varies with the conditions of the case. With an insulated cable this time increases with the thickness of the insulating material. It may often happen that many hours must elapse before this state is arrived at, *i.e.*, before the calculations of the present part of my paper can be applied. It must be noticed that previous to the establishment of the permanent state the heating effect is less injurious; so that in all cases where there is a considerable amount of insulating material the current may, during the first working hours, be considerably in excess of what has been calculated out here as the working current. The reason of this is, that during this preliminary stage the heat is used up in raising the temperature of the insulating material, which serves to cool the conductor.

2. *Specific Heat.*—This leads me to my second remark about the above calculations, viz., the influence of specific heat of the insulator. It has been explained that, after the permanent state has set in, the heat which is generated in the conductor all passes through the insulator to the external air. But previous to that time the heat generated is partly used up in raising each layer of the insulator up to the temperature which it must have when in permanent state. The quantity of heat used up in this way depends upon the specific heat of the insulator. The specific heat is the number of units of heat required to raise the temperature of one gramme of the material 1° C. This quantity is known for a large number of substances.

A patent has been taken out for resistances of fine wire through which large currents can be made to pass without undue heating, by embedding the wire in cement or plaster of Paris. The cooling effect of the plaster of Paris is dependent upon its specific heat, and is only temporary. After a very long run of a current through such a conductor, the heating may become greater than in air; and, if the temperature be that of red heat, the plaster becomes a good enough conductor to lower the resistance of the combination so as to make it useless.

I have known of cases where much larger currents have been carried through cables than would be possible by the formula: it is probable in these cases that the current was not continued long enough for the permanent distribution of the temperature to be arrived at. Hence the wire carried a larger current without overheating.

3. Let us form some estimate of the work which is required to heat the insulator to its permanent condition. The exact solution of this problem is troublesome, so we must be content with a very general view of the question. If r_1 and r_2 be the radii of the interior and exterior respectively of the insulator, the mass of this material in a centimètre length is $\pi (r_2^2 - r_1^2)$. Its weight is $w \pi (r_2^2 - r_1^2)$ when w is the specific gravity of the insulator. The heat required to raise its temperature 1° C. is $c w \pi (r_2^2 - r_1^2)$ when c is the specific heat of the material. I can find no determination of the specific heat of gutta percha, but, by the

analogy of the substances which it most resembles, it is probably about 0·2. We may take this value for the present, remembering that it is desirable to have experiments made upon all the substances used as insulators, so as to know their specific heats. The density of gutta percha is about 1·0. If we take, as an example, the data derived from an experiment in which $C = 500$, $r_1 = \cdot 625$, $r_2 = 5$, we find

$$c\,w\,\pi\,(r_2^2 - r_1^2) = 0{\cdot}2 \times 1{\cdot}0 \times 3{\cdot}1416 \times 24{\cdot}6$$
$$= 15{\cdot}3 \text{ heat units per centimètre length.}$$

And if it is raised on an average 25° C. it requires $25 \times 15{\cdot}3$ heat units per centimètre length to establish the state of steady flow.

Now let us see what time it required to generate this heat with the current of 500 ampères.

The specific resistance of copper is ·000001624 ohm at 0° C., $\therefore$ the resistance of one centimètre length of the specified cable is $\frac{\cdot 000001624}{\pi\,(\cdot 625)^2}$, and the heat generated by 500 ampères is equivalent to $\frac{\cdot 000001624}{\pi\,(\cdot 625)^2} \times 250{,}000$ watts per centimètre, or $\frac{\cdot 24 \times \cdot 1624 \times 2{\cdot}5}{\pi\,(\cdot 625)^2}$ heat units per second per centimètre $= \cdot 07943$ heat units per second per centimètre. But the heat required to warm up the insulator to its permanent state is 15·3 heat units per centimètre length per degree centigrade. Hence, supposing that all the heat generated goes to warm up that insulator, and that none passes through it until the permanent state has been attained, it will take $\frac{15{\cdot}3 \times 25}{\cdot 079}$ seconds, = 1 hour 21 minutes. It is clear, then, that, since during all this time much heat is passing through, it will be many hours before a current of 500 ampères will be able to heat it as much as is implied by the permanent state.

4. It will be readily believed, from what has been said, how necessary it is to know the thermal conductivity of the insulating material employed in cables. Now, it is very important to notice that the thermal conductivity of substances behaves in the same

way as the electrical. The late Principal J. D. Forbes showed that the metals lie in the same order for either conductivity, and that iron becomes a worse conductor for heat at higher temperatures, just as it does for electricity. Now, in the winter of 1872-3, I measured the conductivities of a large number of substances* by an extremely accurate method, consisting of freezing water through them. All the values are less than those which have been obtained by other experimenters at higher temperatures, and the low thermal conductivities of non-metallic substances at low temperatures is completely in accordance with their electric conductivities. It appears, then, that for the high temperatures in conductors the thermal conductivity will be higher, and a larger current can be carried than that given by the formulæ and tables of this memoir.

A few comparisons between the results of Herschel and Lebour, Peclet, and myself will show this.

	G. Forbes.	Herschel.	Peclet.
Marble	·00115 to ·00177	·00470 to ·00560	·0048 to ·0097
Slate	·00081 —	·00315 to ·00550	
Vulcanised rubber	·000089 —	·00034 to ·00055	
Vulcanite	·000083	·00037	
Caoutchouc	—	—	·00041
Gutta percha ...	—	—	·00048

The average temperature of my results is $-10°$ C; that of the others about $+40°$ C. At about $+2°$ C., Stephan found the conductivity of ebonite (vulcanite) 0·00026.

5. Another point to be considered is, that when exposed to the air the temperature of the outer surface of the insulator is higher than that of the air. If the cable be in water this is not the case, unless excessive currents be used. The case of a cable in water is the most easy to calculate, and is also the most advantageous in practice, as a larger current can be thus conveyed.

* *Proceedings Royal Society, Edinburgh*, 1873.

When it is further considered that under these conditions gutta percha is practically indestructible, we see that in very many cases it will be advantageous to utilise water-power to generate electricity, and the river bed to carry the conductor to the place where the electricity is to be used.

It has often been noticed that the insulation of leads is unaffected by a few hours' run, but is quite hot and soft after twenty-four or thirty hours. This is completely accounted for by what has now been said.

A general result of this investigation is that an electric insulator should have as high a thermal conductivity and as high a specific heat as possible.

Having now discussed fully the conditions of the problem of a cable in air or water, I have computed a table for wires from 1 mm. to 10 cm. diameter, in which the diameter of the insulated cable is four times that of the conductor (this being, as I find from makers' price lists, a common ratio), showing the current which will raise the temperatures t° C. above those of the surrounding air. [This table is substituted for one exhibited to the Society, as it is of more practical value.]

TABLE II.

Subaqueous and Aërial Cables (insulated)—

$$\frac{\text{Diameter of cable}}{\text{Diameter of conductor}} = 4.$$

Temperature of air = 20° C.

t = excess of temperature of conductor over air.

diameter in centimètres and mills.		CURRENT IN AMPÈRES.				
		$t = 1°$.	$t = 9°$.	$t = 25°$.	$t = 49°$.	$t = 81°$.
Cm.	Mills.					
·1	40	3·7	11·0	17·8	24·0	29·5
·2	80	9·1	27·0	43·8	59·0	72·5
·3	120	15·0	44·4	72·1	97·3	119
·4	160	21·2	62·5	102	137	168
·5	200	27·4	81·0	131	177	218
·6	240	33·7	100	164	219	268
·7	280	40·1	119	192	259	319
·8	310	46·4	137	223	301	369
·9	350	52·9	157	253	342	420
1·0	390	59·3	175	285	384	472
2·0	780	124	367	595	803	988
3·0	1180	189	559	908	1225	1503
4·0	1570	254	753	1221	1646	2021
5·0	1970	319	945	1534	2068	2523
6·0	2360	385	1138	1846	2491	3058
7·0	2760	450	1330	2158	2846	3575
8·0	3150	514	1525	2472	3335	4094
9·0	3540	580	1716	2785	3755	4611
10·0	3940	645	1909	3097	4178	5130

Computed from the formula,

$$C = \sqrt{\left\{\frac{\pi^2 D_1^3 K}{\cdot 48\ R} . t \times \frac{3\ D_2}{10 + 3\ D_2 \log_e \frac{D_2}{D_1}}\right\}}$$

K = thermal conductivity of insulator, = ·00048 for gutta percha;
E = coefficient of cooling, = ·0003.

If, as is possible, the thermal conductivity of an insulating covering were only 0·0003, then a change of K to this amount can be approximately allowed for by multiplying the currents in the table by a factor which varies from 0·95 to 0·84 for the first

ten (incl.), and from 0·84 to 0·78 for the last ten sizes (incl.) of conductors.

Sec. 5. Buried Conductors.

The case of conductors buried underground is very difficult to treat mathematically. At present I content myself with a study of a specially favourable case, viz., when the conductor takes the form of a thin sheet lying in a horizontal plane under the ground. This form requires far less metal than any other. The heat which is created by the current is at first largely absorbed in heating up the earth near to it, but after some hours a tolerably permanent state sets in, when a very small amount of heat is still penetrating downwards; but the greater part is conducted through the superincumbent earth and paving, and thence cooled by radiation and convection.

With regard to conduction into the soil, we have some experience from the observations which have been made on the temperature at various depths in the soil or in rock. The daily and yearly variations of temperature produce waves of heat in the soil, which are slowly propagated. At a depth of 25 feet the maximum heat occurs in midwinter, and the annual variation of temperature is only $\frac{1}{23}$ of what it is at the surface.

At a depth of two feet the daily variations of temperature are barely perceptible. In the buried cable the heat generated in the dark hours will not all be dissipated at once, but there will be a steady flow of heat at the surface day and night.

Having stated these preliminary facts, I shall now attempt an approximate solution of our problem.

Problem III.—*A sheet of copper* 1 *centimètre thick and of a width b, buried at a depth d, carries a current* C *with a rise of temperature t above the surface of the ground, the ground being t'° above the surrounding air. When the steady flow of heat has set in, find the relation between these quantities.*

The heat generated per centimètre length per second

$$=\frac{C^2 R \times \cdot 24}{b};$$

The heat radiated* $= \cdot 0003 \times b \times t'$;
and these are equal.

$$\therefore C^2 = \frac{b^2 t'}{800 R}.$$

It would not be permissible to have the surface of the ground raised more than 5° C. in summer. Let us take 10° as a maximum value for t', $\therefore C^2 = \dfrac{b^2}{80 R}$.

We have also the equation of conductivity (N.B.—Conductivity of paving stones and similar materials is ·004 to ·005, according to the experiments and deductions of Peclet, Herschel and Lebour, J. D. Forbes, Thomson and Everett, Ayrton and Perry). $\dfrac{C^2 R \times \cdot 24}{b} = \text{heat conducted} = \dfrac{K b t}{d} = \cdot 004 \times \dfrac{b t}{d}$

$$\therefore C^2 = \frac{\cdot 004 \times b^2 t}{\cdot 24 R \cdot d} = \frac{b^2}{80 R}$$

$$\therefore \frac{\cdot 004 t}{\cdot 24 d} = \frac{1}{80} \qquad t = \cdot 75 \times d.$$

With a depth of 2 feet, or 60 centimètres, the rise of temperature from the surface of the earth to the conductor is 45°, and the difference of temperature between the conductor and surrounding air is 55° C. We might place the conductor under the foot pavement or street close to the surface. This would diminish the value of t, but practically we are not injured by taking a depth of 2 feet, as 50° C. is a permissible rise of temperature. Up to this depth, then, we need only consider the rate at which we can get rid of heat by cooling. We have the equation

$$C^2 = \frac{b^2}{80 R}$$

and for 50° above the temperature of the air, assumed to be 15°, we have

$$R = 2{\cdot}031 \times 10^{-6} \text{ ohm},$$

$$\therefore C = \frac{b \times 10^3}{\sqrt{162{\cdot}48}} = 25\, b.$$

---

* It is assumed that the coefficient of cooling for the ground is the same as for a ball freely suspended. It is probably less.

The following table is calculated on these principles:—

TABLE III.

Underground flat conductor of copper 1 cm. thick at a depth less than 2 feet below the surface, raising temperature of conductor less than 50° C., and surface of ground 10° C. above air.

Width of copper conductor 1 cm. thick.	Equivalent diameter of same section.	Current to raise surface temperature 10° C.
Cm.	Cm.	Ampères.
10	3·5	250
40	7·0	1,000
90	10·5	2,250
160	14·0	4,000
360	21·0	9,000
2,800	67·5	70,000

The second column is given to show the diameter of wire which would give the same quantity of metal, merely to admit of a comparison with the aerial conductors.

It appears, then, that if $c = 70{,}000$ ampères, we must have $b = 2{,}800$ centimètres, or the strip of copper must be 28 mètres wide. This gives the least weight of copper permissible (unless, indeed, we were to diminish still further the thickness), and it gives us a section = 53 centimètres square of pure copper, equivalent to a diameter of 67·5 centimètres.

I wish here to insist very positively upon the necessity of making such conductors for large currents in the form of flat sheets. Further, when we see the enormous mass of metal required, it is clearly necessary to use iron, which reduces the cost of material to about $\frac{1}{6}$. I hold this view very strongly in opposition to my friend Mr. Preece, who maintains that for electric light leads very pure copper should be used.

SEC. 6. COILS.

There is a very important application of the principles which have here been enunciated, and this is to the currents which can be carried by coils of wire when the wire is varied in thickness or the coil is varied in size. I have been sometimes simply astounded to see the waste of labour, time, and money which have

been lavished on determining the proper thickness of wire with which to wind a coil without overheating, for a dynamo field-magnet, for regulating coils in arc lamps, etc., while the whole thing could be done by calculation. My first experiments to test the truth of the theory were undertaken two years ago. I first attacked the question as follows:—Having two coils of the same size, but rolled with wires of different diameters, so as to occupy the same volume and to have the same weight, then if the number of turns on each bobbin is considerable, the length of wire is proportional to $\frac{1}{D^2}$ (D being the diameter of the wire), and the cross-section is proportional to D^2, hence the resistance of each coil is proportional to $\frac{1}{D^4}$, and the heat developed in the circuit during a unit of time varies as $C^2 R$ or $\frac{C^2}{D^4}$. But the radiating surface of the two coils is the same; whence, if the temperature be the same in both, the heat lost by radiation and convection in a given time is constant, and the heat created in unit time must be constant.

Whence
$$\frac{C^2}{D^4} = a^2, \text{ where } a \text{ is constant,}$$
and
$$C = a\,D^2;$$

i.e., to attain the same temperature with equal-sized bobbins, the current varies as the section of the wire.

To test the truth of this law, I prepared two copper tubes with flanges at each extremity, and closed at one end, and wound equal weights of two kinds of wire of different diameters. Filling one with water, and inserting a thermometer, the current was increased very slowly until a fixed temperature, sufficiently high, was steadily maintained. The tangent galvanometer was then read off. Afterwards the same temperature was attained with the other coil in the same way. The law above stated was exactly confirmed: the tangents of the angles were proportional to the squares of the diameters.

I next tested the theory in the case of the following problem:—*Two coils of similar shape have their linear dimensions in the*

ratio $n:1$, and the thickness of wire in the same ratio: what ratio of currents is required to raise both to the same temperature?

Let C R H be the current, resistance, and heat generated in the smaller coil, and C′ R′ H′ the corresponding quantities in the larger coil.

Then we have

$$C^2 R = a H, \text{ when } a \text{ is a constant.}$$

$$C'^2 R' = a H'$$

$$R' = \frac{R}{n}$$

Also, since the temperatures are equal, the heat radiated and convected in the two cases is in proportion to the surfaces, or as $n^2 : 1$, and this is equal to the heat generated, wherefore

$$H' = n^2 H,$$

or

$$C'^2 R' = n^2 C^2 R,$$

or

$$C'^2 \frac{R}{n} = n^2 C^2 R,$$

and

$$C'^2 = n^3 C^2;$$

i.e., the squares of the currents are proportional to the cubes of the linear dimensions. To test this, Mr. R. Goodwin made for me the following experiments:—Coils of wire were wound upon two bobbins whose dimensions are—

Small coil—Length = 50 mm.;
Diameter of tube = 13 mm.;
Diameter of wire = 1·05 mm.

Large coil—Length = 100 mm.;
Diameter of tube = 25·7 mm.
Diameter of wire = 2·09 mm.

Here $n = 2$ and $n^{\frac{3}{2}} = 2{\cdot}83 = \frac{C'}{C}$ theoretically.

At the temperature of 63° C., the tangent galvanometer showed a deflection of 37° with the small coil, and at the same temperature with the large coil the deflection of 64°. The tangents of these angles are 0·75355 and 2·0503 respectively, the ratio of which is 2·72 : 1, which agrees well with the theoretical value.

It becomes an easy thing in any case to calculate the heating of a coil when we know its cooling surface and its resistance.

Let ρ = the resistance of a coil in ohms at the permissible temperature,

S = the surface exposed to the air measured in centimètres,

t = the rise in temperature,

C = the current in ampères.

$$\cdot 24\, C^2 \rho = \text{heat generated} = E\, t\, S,$$

where E is McFarlane's constant varying from ·0002 to ·0003. The latter value may be taken. If 50° C. be the permissible rise in temperature

$$C = \sqrt{\frac{\cdot 0003 \times 50 \times S}{\cdot 24 \times \rho}} = \cdot 25 \sqrt{\frac{S}{\rho}}$$

N.B.—It must be remembered in practice that the resistance (cold) must be increased by $\frac{1}{5}$ of its value to give ρ.

Example:—The resistance of the field-magnets of a dynamo is 1·5 ohms cold, and the surface exposed to the air is 1 metre: find the current to heat it not more than 50° C. Here $S = 10{,}000$, $\rho = 1\cdot 8$ ohms, and $C = \cdot 25 \sqrt{\frac{10{,}000}{1\cdot 8}} = 33\cdot 5$ ampères. Those who are accustomed to handling dynamos will know that this is very much what we actually find, and it gives us confidence in the applications of theory.

Sec. 7. Conclusions.

The general result of this research seems to be that we have the factors required for arriving at correct theoretical results. The general accordance of the theory with the few experimental facts which I have been able to get hold of, give a confidence in the application of these principles.

1. One of the most important results I have obtained is that the insulation of an aerial conductor is favourable, and gives us a power of using larger currents with conductors of the same size, when the diameters are not very great.

2. In small installations, the question of safety from fire or

injury to insulation is not likely to crop up, but the tables here calculated will always be useful to let us know the amount of heating.

3. It is satisfactory to have a set of tables which will give us the increase of temperature with any arrangement of currents and size of wires. But these tables which I have worked out are not (except in the case of large currents) a measure of the best size of conductor for any special installation. They indicate only the safety from heating.

4. In buried conductors the mass of metal must be very great indeed, the inferior limit being set by the amount of heating of the surface of the ground which is permissible.

5. In all ordinary applications of coils of wire we can calculate the rise of temperature, experiment and theory being found to be in accordance.

The PRESIDENT: I am afraid that the Council have made a mistake in putting down two papers to be read this evening; but the Committee were unaware of the length of Professor Forbes's paper, and unfortunately for members it is not in the state in which, like Mr. Blakesley's paper, it can be looked over beforehand. But it has been suggested that Professor Forbes's paper should be printed, so that it may be in the hands of members before the next meeting, and we have already Mr. Blakesley's paper in slips. Then the whole subject can be discussed better than it can be treated this evening. As it is now ten o'clock I will, with Mr. Blakesley's permission, also postpone his paper until our next meeting.

A ballot took place, at which the following were elected:—

Members:

J. Farquharson.	D. W. Lane.

Associates:

E. W. Brown.	Chas. Jno. Phillips.

The meeting then adjourned until 24th April, 1884.

THE LIBRARY.

ACCESSIONS TO THE LIBRARY TO MAY 1, 1884.

By Alfred J. Frost, *Librarian.*

[*Works marked with an asterisk* (*) *have been purchased.* G. J. Symons, Esq., F.R.S., *has presented to the Library a small collection of papers on Magnetism, etc., from the library of the late* General Sir E. Sabine, F.R.S., D.C.L.: *these works are marked thus* †]

Acland [Hy. W.] [*Vide* Radcliffe Library.]

* **Adams** [Geo.] Lectures on Natural and Experimental Philosophy, considered in its present state of improvement. Second edition, with considerable corrections and additions by Wm. Jones. 5 vols. 8vo. *London*, 1799

† **Admiralty.** Practical Rules for ascertaining those Deviations of the Compass which are caused by the iron in the ship. 8vo. 12 pp. *London*, 1845

Ainsworth. [*Vide* Sabine, E.]

† **Angström** [Dr. A. J.] Magnetische Beobachtungen bei Gelegenheit einer Reise nach Deutschland und Frankreich im Jahre, 1844. 4to. 14 pp. Plate. [Akad. d. Wiss, V. Bd. I. Abth.]

American Bell Telephone Company *v.* **Amos** E. Dolbear. Pleadings, Testimony, Exhibits, Arguments, Opinions, Decree, Injunction. La. 8vo. 914 pp. *Boston* [*U.S.*] 1884
[Presented by J. J. Storrow, Esq.]

American Electrical Society. Journal. 8vo. 5 parts (3 vols.) 8vo. *Chicago*, 1875-80

Anglo-American "Brush" Electric Light Corporation, Limited. Price List No. 749. 4to. *London*, 1884
[Presented by W. M. Mordey, Associate.]

* **Arago** [François]. Biographies of Distinguished Scientific Men. 8vo. 607 pp. Translated by Admiral W. H. Smyth, D.C.L., F.R.S., the Rev. Baden Powell, M.A., F.R.S., and Robt. Grant, M.A. *London*, 1857

Armengaud jeune [M.] **et Vanoni** [M. L.] Visite à l'Exposition Internationale d'Électricité de Vienne, 1883. 8vo. 38 pp. [Ext. du *Journal L'Electricité* des 3, 10, 17, et 24 Nov., Vol. 6, 1883.] *Paris*, 1883
[Presented by J. Aylmer, Esq., Local Hon. Sec.]

Atlantic Telegraph. Rules to be followed in the Electrical Department, from the sailing of the ships to lay the cable till the end of the operation. Signed by Wm. Thomson and E. O. Wildman Whitehouse. 4to. 1 p. *Plymouth*, 1858
[Presented by Sir F. Bolton, Hon. Sec.]

Atlantic Telegraph Co. Special Signals to be used during the laying of the Atlantic Telegraph, 1866. Card. 1866
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* **Baird** [Spencer F.] [Editor]. Annual Record of Science and Industry for 1873. 8vo. 714 pp. *New York*, 1874

Banneux [M. J.] Télégraphie et Téléphonie Simultanées par les mêmes fils conducteurs (Système F. Van Rysselberghe). 8vo. 22 pp. *Berne*, 1884

† **Barlow** [Peter]. A Report, &c., containing an account of the experiments made on board H.M. ships "Leven," "Conway," and "Griper," for correcting the local attraction of those vessels. 8vo. 64 pp. *London*, 1824

Bell [Alex. G.] Speaking Telephone Patent, March 7th, 1876: the Misconceptions and Failures which preceded it. Reis & Bourseul. 1876

——— [*Vide* American Bell Telephone Company.]

Belsham [Thos.] Lectures on Electricity. [M.S. shorthand.] 18mo. 87 pp. *London*, 1827 (?)
[Presented by H. A. Burrows, Associate.]

Berly [J. A.] "Universal Electrical Directory and Advertiser," 1884. Third edition. 8vo. 228 + 273 + 72 + 15 pp. *London and New York*, 1884

Bourseul [*Vide* Bell, A. G.]

† **Brooke** [C.] An Account of the remarkable Magnetic Disturbance which continued from the 22nd to the 25th October, 1847. 8vo. 2 pp. [*Phil. Mag.*, January, 1848.] *London*, 1848

Burq [V.] Choléra presérvation et traitément. 8vo. 8 pp. *Paris*, 1854

Cambridge Philosophical Society. [*Vide* Rayleigh, Lord.]

* **Campbell** [H.] Deafness: its various causes, and their successful removal by Electrolysis. 12mo. 102 pp. *London*, 1880 [?]

† **Cayley** [A.], F.R.S. Rough Note on Magnetic Variations. 8vo. 4 pp. [Not published.] *London*, 1862

* **Chambers's Journal**, April. Curiosities of the Electric Light. 8vo. 4 pp. *London and Edinburgh*, 1884

* **"City,"** Nov. 4, 1882. Cartoon on Electric Light, entitled, "A Gas Director's Dream," with Short Biographies of the Principal Inventors, etc., of the Electric Light. *London*, 1882

Cornell University, The Library of. Published quarterly. *Ithaca*, 1882

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Du Hamel [J. B.] De corporum affectionibus cum manifestis tum occultis. Sm. 8vo. 556 pp. *Parisüs*, 1670

† **Duperrey** [L. J.] Observations du pendule invariable, de l'inclinaison et de la declinaison de l'aiguille aimantée, faites dans la campagne de la corvette de S.M. "La Coquille" pendant les années 1822–25. 8vo. 32 pp. *Paris*, 1827

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* **Electric Telegraph Company** *v.* **Nott, Gamble, and others.** Chancery Proceedings. 4to. 1846

* **Fahie** [J. J.] A History of Electric Telegraphy to the year 1837. Chiefly compiled from original sources and hitherto unpublished documents. 8vo. 542 pp. *London*, 1884

† **Faraday** [Michael], F.R.S. On the Magnetic Affection of Light, and on the Distinction between the Ferromagnetic and Diamagnetic Conditions of Matter. 8vo. 13 pp. [*Phil. Mag.*, September and October, 1846.] *London*, 1846

—— On the Diamagnetic Conditions of Flame and Gases. 8vo. 21 pp. [*Phil. Mag.*, December, 1846. Pp. 401 to 421.] *London*, 1847

Fisher [Rev. Geo.], F.R.S. [*Vide* Home, Sir E.]

Geuns [P.] Memoire sur les aimans. Ou description succinte de tout ce qui est necessaire pour la construction de toute forte d'aiman imaginable. Sm. 8vo. xxiv. + 143 pp. Plates. [Rare.] *Venlo*, 1768
[Presented by Professor Silvanus Thompson, Member.]

Gomez y Pallete [José]. Historia y progresos de la Electricidad. 12mo. 48 pp. *Madrid*, 1884

* **Gudman** [L.] Bulletin donnant d'après les documents officiels la liste compléte de tous les brevets délivrés dans le monde ensier pour des inventions concernant l'électricité. Two sheets. *Paris*, 1882

* **Guillemin** [Amédée]. The Applications of Physical Forces. Translated from the French by Mrs. Norman Lockyer, and edited, with additions and notes, by J. Norman Lockyer, F.R.S. La. 8vo. 741 pp. *London*, 1877

Hamel. [*Vide* Du Hamel.]

* **Hammond** [Robt.] The Electric Light in our Homes. 8vo. 188 pp. *London*, 1884

† **Home** [Sir Everard], Bart., R.N., F.R.S. Magnetical Observations made in the West Indies, on the North Coast of Brazil, and North America, in the years 1834, 1835, 1836, and 1837. Reduced by the Rev. George Fisher, M.A., F.R.S. 4to. 7 pp. Read November 23rd, 1837. [*Phil. Trans.*, Part II., 1838.] *London*, 1838

† **Hansteen** [C.] Beobachtungen über die Intensität des Magnetismus im Nördlichen Europa. 8vo.
[*Annalen der Physik.* Jahrgang, 1825.
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† —— Isodynamische Linien für die ganze Magnetische Kraft. 8vo. iii.–16 pp. iv.–18 pp. [*Ann. d. Phys.*] 1827

† —— Interpolationsformler for Magnetnaalens Misvisning og Helding for forskjellige Punkter i Europa. 8vo. 41 pp. *Christiania*, 1844 (?)

† —— On the Amplitude of the Daily Variation of the Magnetic Dip in Christiania between 10 a.m. and an hour before sunset, from 1844 to 1859. 8vo. 6 pp. [*Proc. Roy. Irish Academy*, May 27th, 1861.] *Dublin*, 1861

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[Presented by the Institution.]

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* **International Fisheries Exhibition.** An Illustrated Description of the Electric Light Machinery in the Exhibition, with Elementary Notes on the Production of Electric Currents. 8vo. 31 pp. *London*, 1883

Iron and Steel Institute. Journal. No. II. 1883. 8vo. 889 + LXIV. pp. *London*, 1884

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* **Klein Franz** [Redacteur.] Bericht über die Internationale Elektrische Ausstellung Wien, 1883, unter mitwirkung hervorragender fachmänner. Herausgegeben vom Niederösterreichischen Gewerbe-Vereine. I. Lieferung. La. 8vo. *Wien*, 1884

† **Kreil** [K.] Letter to M. Kupffer, Director-General of the Physical Observatories in Russia, containing a Succinct Account of the Principal Results of Magnetic Observations at Milan. 8vo. 10 pp. [*Phil. Mag.*, April, 1840, pp. 241 to 250.] *London*, 1840

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* **Lock** [C. G. W.] Workshop Receipts. Third Series. 8vo. 480 pp. *London*, 1884

* **Lockwood** [Thomas D.] Electricity, Magnetism, and Electric Telegraphy: a Practical Guide and Handbook of General Information for Electrical Students, Operators, and Inspectors. 8vo. 377 pp. *New York*, 1858

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† **Loomis** [E.] Additional Observations of the Magnetic Dip in the United States. 4to. 11 pp. Read October 14th, 1839. [*Trans. Amer. Phil. Soc.* New Series, Vol. VII.] *Philadelphia*, 1841

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† ——— On the Variation and Dip of the Magnetic Needle in the United States. 8vo. 10 pp. [Communicated to the Conn. Acad. of Arts and Sciences, and read April 28th, 1840.] *Connecticut* (?), 1840

† ———————————————— 8vo. 24 pp. [Conn. Acad. of Arts and Sciences, April 28th, 1842.] *Connecticut* (?), 1842

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* **Mach** [T. von]. Technisches Wörterbuch fur Telegraphie und Post. 8vo. 395 pp. *Berlin*, 1884

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* **Mathieson** [F. C.] The Electric Light Companies. Second Edition. 1 sheet. *London*, 1883

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[Presented by Capt. Beresford, Member.]

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[Presented by Dr. Lemon.]

——— Seventeenth Annual Report of Telegraph Department, by C. Lemon. Fo. 15 pp. Plates. 1881
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[Presented by Dr. Lemon.]

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[Presented by T. J. Wilmot, Associate.]

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* **Pogliaghi** [P.] La Trasmissione Elettrica dell' Energia. 8vo. 213 pp. [*Biblioteca Scientifica Internazionale*, Vol. XXXVIII.] *Milano*, 1884

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* **Révérend** [A.] Annuaire de l'Électricité. 8vo. 452 pp. *Paris*, 1884

* **Rogers** [Fairman]. The Magnetism of Iron Vessels; with a short Treatise on Terrestrial Magnetism. 18mo. 125 pp. [Van Nostrand's Science Series, No. 30.] *New York*, 1877

† **Royal Society.** Proceedings of the Committee of Physics, including Meteorology, Nos. 1 to 4. February 10 to June 2, 1842. 8vo. 37 pp. *London*, 1842

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† **Sabine** [General Sir E.] On Irregularities observed in the direction of the Compass Needles of H.M.S. "Isabella" and "Alexander" in their late voyage of discovery, and caused by the attraction of the iron contained in the ships. 4to. 11 pp. Plate. [*Phil. Trans.*] *London*, 1819

† ——— Observations on the Variation made on shore or on the ice, at a sufficient distance from the ships to be beyond the influence of the iron which they contained. 4to. 32 pp. [*Phil. Trans.*] *London*, 1819

† ——— Observations on the Dip and Variation of the Magnetic Needle and on the Intensity of the Magnetic Force, made during the late voyage in search of a North-West Passage. 4to. 15 pp. [*Phil. Trans.*] *London*, 1819

† ——— Experiments to ascertain the Ratio of the Magnetic Forces acting on a Needle suspended horizontally in Paris and in London. 4to. 14 pp. [*Phil. Trans.*, June, 1827.] *London*, 1827

† ——— On the Dip of the Magnetic Needle in London in August, 1828. 4to. 7 pp. [*Phil. Trans.*] *London*, 1829

† **Sabine** [General Sir E.] Report on the Phenomena of Terrestrial Magnetism: being an Abstract of the "Magnetismus der Erde" of Prof. Ch. Hansteen. 8vo. 30 pp. [*Brit. Assoc. Report* for 1835.] *London*, 1836

† —— Magnetic Observations. 8vo. 32 pp. [No date.]

† —— Observations on the Direction and Intensity of the Terrestrial Magnetic Force in Scotland. 8vo. 23 pp. 1836

† —— Report on Mr. Ainsworth's Observations on the Magnetic Intensity of the Earth, made during a Journey across the Continent of Europe in 1838. 8vo. 24 pp 1838

† —— Comparison of the Magnetic Lines of No Dip and of Least Intensity. 8vo. 2 pp. [*Phil. Mag.*, Feb., 1839.] *London*, 1839

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† **Siemens** [C. W.], F.R.S. Observations on the Electrical Resistance and Electrification of some Insulating Materials under pressures up to 300 atmospheres. 8vo. 7 pp. [*Brit. Assoc. Reports*, 1863, pp. 688 to 694]. *London*, 1863

* **Société Française de Physique.** Collection de Memoires relatifs à la Physique. Tome I. Memoires de Coulomb. 8vo. 414 pp. *Paris*, 1884

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* **Swinton** [Alan A. Campbell]. The Principles and Practice of Electric Lighting. 8vo. 172 pp. *London*, 1884

Tawse [John P.] Electric Lighting and its Relation to Risk of Fire. 8vo. 28 pp. [Trans. of the Insurance and Actuarial Society of Glasgow. No. 10. Pp. 189-216.] *Glasgow*, 1884

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* **Urquhart** [J. W.] Electrotyping. A Practical Manual, forming a New and Systematic Guide to the Reproduction and Multiplication of Printing Surfaces and Works of Art by the Electro-deposition of Metals. 8vo. 226 pp. *London*, 1881

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* **Vienna Electrical Exhibition, 1883.** Two Photographs of British Section. 1884

Vienna Exhibition, 1883. [*Vide* Klein.]

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Walker [E. O.] Record of Observations of Earth Currents, August, 1882, to October, 1883. M.S. *Belgaum, India*, 1883

* **Welch** [E. J. Cowling]. Table of the Relative Weights and Sizes of Insulated Copper Conductors for conveying Electrical Currents of from 1 to 2,741 ampères. Card. *London*, 1884

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† **Wilde** [H.] On a Property of the Magneto-electric Current to control and render Synchronous the Rotations of the Armatures of a number of Electro-magnetic Induction Machines. 8vo. 9 pp. [*Phil. Mag.*, Jan., 1869.] *London*, 1869

* **Wilson** [Fredk. J. F.] Stereotyping and Electrotyping. 8vo. 195 pp. *London*, 1884

† **Zantedeschi** [Professor]. On the Motions presented by Flame when under the Electro-magnetic influence. 8vo. 4 pp. [*Phil. Mag.*, Dec., 1847. Pp. 421 to 424.] *London*, 1847

Zetzsche [Ed.] Der Typendruck-Telegraph von Dr. Alexander Lucchesini in Florenz. La. 4to. 8 pp. and plate. *Florenz*, 1884

ORIGINAL COMMUNICATION.

LIGHTNING INFLUENCE ON BOARD SHIP.

By Captain Creak, R.N.

I am sending you a case which has occurred in one of the Pacific Steam Navigation Co.'s ships, which may interest you; and as her standard compass is placed, as regards the iron foremast, similarly to what ours is as regards our mizenmast, it seems a subject worth attending to, and I should be much obliged if you would give me your opinion on the matter when you have time.

The "Columbia" was struck by lightning, the current running down the lightning-conductor of the foremast, tearing away the lightning-conductor in one place. The wheel-house is just before the foremast; and on the deck above it, and about eight feet before the foremast, is the standard compass. The wheel-gearing, chains, brackets, barrel, and break are all iron. Two steering compasses are in the fore part of the wheel-house, about four feet from the wheel on each side. Cutlasses and rifles are hung on the after-bulkhead. After the ship was struck, the steering compasses became unmanageable, flying round. Afterwards they were found to be quite untrustworthy, having on some points as much as 5 points deviation. The wheel and gearing, cutlasses and rifles, were all converted into powerful magnets. The ship was swung about three months afterwards. I attended whilst she was being swung, and suggested that she should be taken round both ways, which was done. The whole of the wheel-gearing was passed through the fire in the factory, but the brackets, break, etc., still continued to be magnets, although not so powerful as before. It was then decided to replace them by new ones, which was done. When I was on board in November,

some four months after the storm, the cutlasses were still strong magnets—the rifles also, but to a less degree.

I am inclined to think the foremast is strongly magnetised still, and should very much like if you would give me the opinion you arrive at after looking over the evidence I have sent. I could not find any transverse beams showing magnetism, but some of the bolts at the side of the ship were evidently affected.

To W. H. Preece, Esq., F.R.S.

ABSTRACTS.

G. LE GOARANT DE TROMELIN—NEW DEAD-BEAT GALVANOMETER.

(*Comptes Rendus*, *T.* 97, *No.* 19, *Nov.* 5, 1883, *p.* 995.)

This instrument is a modification of an astatic galvanometer with three needles. Instead of being straight, however, horse-shoe magnets are used, with legs very close together. These three fixed magnets are placed horizontally one above the other, at a distance of five millimètres. The movable coil surrounds the two poles of the middle magnet, leaving sufficient room for it to oscillate freely through twenty degrees on either side. The wire of this small coil is very light, and the connection is made by means of the suspension, as in Thomson's siphon recorder.

If this galvanometer be joined up to the terminals of a telephone from which the vibrating disc has been removed, a deflection of the coil can be caused by allowing a small atom of iron-filing, weighing only a few milligrammes, to fall on to the pole of the magnet in the telephone, a proof of the extreme sensitiveness of the instrument.

It is perfectly dead-beat, for if the two terminals of the galvanometer are joined by a wire of very small resistance, the coil having been deflected from its position, it stops dead at the zero point.

On examining the position of the lines of force, with reference to the four sides of the square, it is evident that the electro-magnetic induction acts on the four sides of the coil, and in the same direction.

G. FOUSSEREAU—RESISTANCE OF SOME INSULATING SUBSTANCES.

(*Comptes Rendus*, *T.* 97, *No.* 19, *Nov.* 5, 1883, *p.* 996.)

The author has pursued further the researches of which an abstract was given in No. 48 of the Journal. In experimenting with porcelain tubes, he found results similar to those obtained with flint-glass, and states that the resistance per cubic centimètre in millions of megohms is 751 at 60° C., and 0·052 at 180° C.

To study the behaviour of sulphur, some of this substance was fused in a test-tube, into which dipped two zinc electrodes of cylindrical form, placed one within the other. In this way the resistance of the intervening layer only was measured, and any disturbing influences of the glass tube were eliminated. With sulphur previously melted, and then allowed to crystallise, there was considerable conductivity about the point of fusion. The values are 7·39

million megohms at 112·1° C., and 3,930 at 69° C.; below this the resistance becomes infinite. It seems that sulphur in the crystalline form has the least conductivity. In passing to the liquid state the resistance becomes forty times less. Between 114° C. and 150° C., the resistance varies inversely as the temperature, in the proportion of 9 to 1. On going beyond 160° C., the thermometer for a time rises much more slowly than before, whilst a change occurs in the colour and consistence of the mass. The resistance which had regularly diminished, at once increases. On cooling down again to the melting point, the resistance is much greater than it was at the same point when the temperature was being raised. After several heatings and coolings, the resistance at the melting point can be made twelve times its original value.

The resistance of solid phosphorus is much less than that of any of the preceding substances: it gave a value 84,000 megohms at 15° C., diminishing to 15,600 megohms at 42° C. With liquid phosphorus in a U tube under carbonic acid gas, and with platinum electrodes, the author found the specific resistance per cubic centimètre to be 2·3 megohms at 25°, and 0·34 megohm at 100° C.

P. PICARD—RAPID METHOD FOR DETERMINING THE WORK ABSORBED OR GIVEN OUT BY A DYNAMO.

(*Comptes Rendus, T.* 97, *No.* 20, *Nov.* 12, *p.* 1033.)

The author proposes to determine, once for all, a value which he calls the statometric constant of the machine. To effect this, a Prony brake is rigged up on the pulley, and a current is passed through the machine, which is just strong enough to saturate the electro-magnets, and weights are added to the pan of the brake, so that the dynamo is just held still. Then

$$W = \frac{2\pi L P N}{60} = 0,$$

since in this case $N = 0$. We can, however, carry out the operations while suppressing this factor N, and we shall have the expression $\frac{\pi L P}{30}$ obtained with a current of intensity i, or with a current of one ampère, $\frac{\pi L P}{30\,i}$; this is the statometric constant $= K$. Knowing, then, at any time the current in ampères (I) and the revolutions per minute (N), we have at once

$$W = K I N.$$

It is of course understood that the work calculated on the constant K is purely theoretical, and must be increased or diminished by a coefficient accordingly as the dynamo is used as generator or as motor.

V. STROUHAL and C. BARUS—THE GALVANIC TEMPERATURE COEFFICIENTS OF STEEL AND IRON.

(*Annalen der Physik und Chemie, B.* XX., *H.* 3, *No.* 11, 1883, *p.* 525.)

Former experimenters have deduced values for the temperature coefficient for the resistance of steel which differ only very slightly from that of iron. In

the authors' opinion this seemed unsatisfactory, looking at the very different physical properties of steel. It is known that the percentage composition of alloys exerts a considerable influence on the coefficient of change of resistance with temperature which these alloys show; this has been found particularly to be the case with German silver and platinum-silver alloy. As has been shown in other works by the same authors, steel in its behaviour shows a marked resemblance to an alloy of two metals; and it was therefore to be expected that the galvanic temperature coefficient would decrease in a regular way when the resistance increased by hardening. This expectation has been fully realised, but only for limited variation of temperature. The equation used by the authors was

$$S_t = S_0 (1 + \alpha t),$$

where t varied between 10° C. and 35° C. Owing to the very small variations of temperature, and hence the very small changes in the resistance, which only amounted to some hundredths of an ohm, very great care had to be exercised in carrying out the experiments.

To the wire to be tested were fixed at two points, two very fine copper wires, and the intervening portion of the steel wire was placed in a water bath containing either hot or cold water. Distilled water was used, and no short circuit of the two copper wires through the water was observed. Steel wires were used of 1·5 mm. diameter, which, having been hardened previously, were annealed by passing a strong current through them. As the heating proceeded, the colour of the surface indicated very clearly the point to which the annealing had gone, and on interrupting the current the wire for the greater part of its length showed one even colour. The following table will show how exactly the authors' expectations were fulfilled, viz., that the galvanic temperature coefficient of steel varies continuously as the resistance with the degree of hardness of the steel, and increases as the annealing is carried further:—

Condition of steel.	Specific resistance microhms.	Temperature coefficient.
Glass hard	45·7	0·00161
Annealed bright yellow	28·9	0·00244
" yellow	26·3	0·00280
" blue	20·5	0·00330
" bright blue	18·4	0·00360
Soft	15·9	0·00423

A great number of experiments were made with very varying samples of wrought iron, in which it was found that, while the specific resistance in microhms varied from 9·4 to 18·4, the temperature coefficient varied from 0·0052 to 0·0034. With cast iron it was found that the specific resistance was greater than that of the hardest steel, and that consequently the temperature coefficient must come out much lower. Three bars of cast iron were tested, each about 25 cm. long and about 0·4 square cm. section, so that the total resistance was only about 0·004 ohm, rendering necessary the greatest precautions in carrying out the experiments. The values obtained were for specific resistance respectively 76, 76·2, and 83·3 microhms, and the temperature coefficients 0·00124, 0·00138, and 0·00126.

All the experiments point to the conclusion that steel behaves itself much as an alloy would do, and that the results obtained are but consequences of a general law, and that, in all cases where with the same matter changes occur in the galvanic resistance, these also influence the temperature coefficient, and that the two vary inversely.

E. WIEDEMANN—THE CONNECTION BETWEEN FRICTIONAL AND ELECTRICAL RESISTANCE OF SOLUTIONS OF SALTS.

(*Annalen der Physik und Chemie, B. XX. H.* 3, *No.* 11, 1883, *p.* 537.)

In former experiments on this point, solutions of various degrees of concentration have been employed; but, in the author's opinion, a better plan is to make use of solutions of the same salt in various media, which should have a very different coefficient of friction. He employed two solutions, one of sulphate of zinc in water, the other, sulphate of zinc in a mixture of water and glycerine. The frictional resistance was determined by Sprung's method (*Annalen* 159, p. 1, 1876), and the electrical resistance by a Wheatstone bridge, in which equally concentrated solutions in water and in glycerine formed the two branches. Ten parts $Zn\ SO_4 + 7\ H_2\ O$ were dissolved in 100 parts water, and then 50 cb. cm., 100 cb. cm., and 250 cb. cm. of this solution were taken, and in each case diluted with water or glycerine to 500 cb. cm. The ratios found were—

	Frictional resistance.	Electrical resistance.
50 $Zn\ SO_4$	1: 68·7	1: 12·1
100 $Zn\ SO_4$	1: 29·8	1: 9·52
250 $Zn\ SO_4$	1: 6·15	1: 3·68

in which the higher figure always belongs to the glycerine solution. Stronger solutions were also tried, but the final conclusion remained the same, viz., that no proportion exists between the frictional and the electrical resistance, but that the medium in which the salt is dissolved has great influence.

D. KAEMPFER—THE MEASUREMENT OF ELECTRICAL FORCES BY MEANS OF THE ELECTRICAL WHIRL.

(*Annalen der Physik und Chemie, B. XX., H.* 4, *No.* 12A, 1883, *p.* 601.)

The apparatus used consisted of a stout brass wire, the ends of which were bent at right angles in an opposite direction and sharpened to conical points. This electric whirl was suspended by a platinum wire 270 mm. long, and 0·114 mm. thick, and was provided with a pointer made of ebonite, which moved over a divided circle. On connecting the knob at the top of the suspension with the prime conductor of an influence machine, the negative conductor being to earth, and working the machine, the whirl was rotated through a certain angle and came to a position of rest, in which the force of the electricity outflowing from the points was counterbalanced by the torsion of the wire. The coefficient of torsion of the wire was found experimentally

and taking into account the radius of the whirl, it was found that a source of electricity which was held in equilibrium by a torsion of one degree, exerted a mechanical action equal to that of gravity on a mass of 0·1639 milligrammes. Such a source of electricity therefore represents in absolute measure a force

$$P = g \times 0{\cdot}1639 \text{ mm. mg. sec.}^{-2}.$$
$$P = 1607{\cdot}9 \text{ mm. mg. sec.}^{-2}.$$

With a Holtz machine the angle of torsion was 147·4°, and hence the force in this case was 237,020 mm. mg. sec.$^{-2}$.

According to the theory of electrostatic potential, the tension at the points of an electrified conductor is infinitely great if the conductor has no resistance, and the points are true points. It is of course impossible to attain this ideal condition, but we may approximate very nearly to it. Assuming that in the case of our whirl we obtain this very close approximation, we may deduce a relation between the quantity of electricity and the forces of reaction it produces.

If two sources of electricity, E_1 and E_2, furnish in the same interval of time, under similar conditions, quantities of electricity, Q_1 and Q_2, and if E_1 causes a torsion α, and E_2 a torsion β, then the mechanical work is represented in the two cases by $P\alpha$ and $P\beta$. If now W_1 and W_2 denote the potentials of Q_1 and Q_2 on themselves, and remembering that the potential of a quantity of electricity on itself is equal to the work which the electricity can perform, we have

$$W_1 = P\alpha \text{ and } W_2 = P\beta;$$

or

$$W_1 : W_2 = \alpha : \beta.$$

Further, it can be shown that

$$W_1 : W_2 = Q_1^2 : Q_2^2,$$

and combining the two equations we have

$$Q_1 : Q_2 = \sqrt{\alpha} : \sqrt{\beta}.$$

The quantities of electricity produced in equal times from two constant sources are proportional to the square roots of the angle of torsion, and this will be true whatever theory we accept as explaining the escape of electricity from points.

The author has made many experiments to ascertain the correctness of his conclusions, and the results obtained with Holtz machines, Leyden jars, and induction coils, were all very satisfactory. Amongst other things, he found that the electric whirl served very well for the comparison of potentials.

S. KALISCHER—DOES THE CONDENSATION OF AQUEOUS VAPOUR GENERATE ELECTRICITY?

(*Annalen der Physik und Chemie, B. XX., H. 4, No. 12a*, 1883, *p.* 614.)

This question is an important one in its meteorological aspect, and the author has been led to undertake his experiments since he thinks that those which have been made by several experimenters are faulty in that the aqueous vapour experimented upon was produced by evaporation. In such a case the

steam generated carries off with it particles of water and dust, and the friction of these on the sides of the containing vessel would account for the apparent production of electricity which has been observed.

He proceeds, therefore, in a different way, by condensing the vapour held in natural suspension in the air. Twelve large beaker glasses, covered externally with tinfoil, were placed on a sheet of tinned iron plate, which lay on a sheet of glass supported on blocks of paraffin. The whole stood in a metal case connected to earth; a wire from the iron plate was joined up to the one pair of quadrants of a Kirchhoff electrometer, the other pair being to earth; the greatest care was taken with the insulation, and it was found that the charge of the electrometer after twenty-four hours had scarcely diminished. On placing ice or a freezing mixture in the twelve beakers, the vapour in the air, which had free access to them through the wire gauze cover of the metal case, was condensed on the outside. A great number of observations were made, and, though the results varied very much, it was found that—

1. The deflections of the electrometer needle were of the same kind and value, whether the beakers were filled with ice or not.

2. In both cases the deflections resulted sometimes in one, sometimes in the other direction.

3. The deflections, when the beakers were empty, were at times greater than when they were filled with ice.

Experiments were also tried with air compressed in a vessel under several atmospheres, and then allowed suddenly to escape, when, owing to the cooling due to expansion, any vapour was condensed. All have, however, led the author to the same conclusion, that condensation of vapour does not generate electricity.

V. STROUHAL and C. BARUS—INFLUENCE OF THE HARDNESS OF STEEL ON ITS CAPABILITIES FOR BEING MAGNETISED.

(Annalen der Physik und Chemie, B. XX., H. 4, No. 12a, 1883, p. 621.)

Up to the time of the authors' experiments, our knowledge of this question may be summed up thus:—Glass-hard steel magnets can take up a greater amount of permanent magnetism than annealed bars, so long as the relative dimensions remain below a fixed critical point, which is constant for different kinds of steel; above this value the glass-hard magnets are surpassed by the annealed ones. This problem has been attacked by many physicists, and has been resolved qualitatively, but not quantitatively. One of the chief reasons for this is that the degree to which the annealing was carried was judged of by the colour, and therefore was not very precise. Since, however, the authors have worked out accurately the relation between the hardness of steel and its specific resistance, and coefficient for changes of resistance with temperature (see Abstract above), it is possible to ascertain accurately the degree to which the annealing has been carried. Another reason is that magnets of very different diameters were compared with each other, and it is more than

probable that with thick bars the process of annealing may produce molecular changes in the centre which would not arise with thin bars.

The metal experimented upon was English silver steel, supplied by Messrs. Cooks Brothers, of Sheffield, and the wires had respectively the diameters 0·084 cm. and 0·15 cm. It was found, however, that in studying the problem how far the hardness affected the power of magnetisation of the steel, the magnets had to be made from one and the same homogeneous wire, previously hardened. The homogeneity of the wires was conveniently and accurately tested by their resistance.

The wires were magnetised in a solenoid through which a current of 30 ampères generated by a Siemens dynamo machine was made to circulate. The solenoid of a length $2\ a = 22{\cdot}3$ cm. had 10 layers of wire each of $n = 55$ turns, and the radius, r, of the windings varied from 2·1 to 5·3 cm. From these data it was calculated that the magnetising power of the solenoid for different distances from its middle point varied from 884 to 874 $\left[\frac{g^{\frac{1}{2}}}{\text{cm.}^{\frac{1}{2}}\ \text{sec.}}\right]$. The greatest power employed by Ruths with steel bars was 147, so that the authors made use of a far more powerful magnetising coil than had ever before been used in such experiments, and they were well assured that their steel wires were magnetised to saturation. An important point was the way in which the current should be started and stopped. Instead of starting the current with the bar out of the solenoid, and removing the bar before interrupting the current, as had been done by Wiedemann and Fromme, or of closing and breaking the circuit suddenly with the bar in the coil, as Holtz and Ruths had done, the authors started with the bar in the coil with a very small current, which they gradually increased to a maximum and as gradually decreased to nil before removing the bar. This was easily done, as the dynamo was driven by a gas-engine. The current was only passed for 10 or 15 seconds, and after an interval of 30 seconds the process was repeated.

The magnetic moment of the magnetised steel wires was determined in the usual way by means of a magnetometer, the wires being placed in the two general positions relatively to the instrument, the values being calculated from the formulæ,

$$\text{for the first position, } M = \frac{1}{2}\,\frac{r^3\ T\ \tan.\ \phi}{1 + \frac{1}{2}\frac{l^2}{r^2}},$$

$$\text{and for the second, } M = \frac{r^3\ T\ \tan.\ \phi}{1 - \frac{3}{8}\frac{l^2}{r^2}},$$

in which T is the horizontal intensity at the place of observation, r the distance of the magnetometer mirror from the axis of rotation of the deflection apparatus, ϕ the deflection, and l the distance apart of the poles of the magnet.

The degree of hardness of the steel was determined, as has been said, by its resistance, which was measured on a modified form of Wheatstone bridge; the dimensions of the wires were then very accurately measured, and from these the specific resistance in microhms at 0° C. was calculated, which was taken as a measure of the hardness.

The wires were first tested in the glass-hard condition; they were then successively exposed to the following temperatures, at which they were kept for several hours:—steam at 100° C.; aniline vapour at 185° C.; melting tin at 240° C.; melting lead at 330° C.; melting zinc at 420° C.; finally the wire was heated to brightness. By these means, and by varying the duration of the annealing in the several baths, twelve degrees of hardness could be produced between glass-hard and soft steel.

The authors give a most extensive series of tables, enumerating the results they obtained, and from which they have plotted a series of curves. One such table may serve as an example, it being premised that by specific magnetism is meant the magnetic moment of unit of mass (1 gramme).

WIRE NO. II.

Annealing.	Hardness measured by specific resistance.	Specific Magnetism. Relative Dimensions. 20	40	60	80	100
Glass-hard	37·3	31·5	45·0	49·5	51·3	52·4
1 hour in steam at 100°	34·7	30·4	43·4	47·8	49·7	50·5
3 hours " "	33·0	30·0	42·8	47·3	49·0	49·9
6 " " "	32·2	29·9	42·7	47·2	48·8	49·8
10 " " "	31·6	29·9	42·7	47·2	48·8	49·7
20 minutes in aniline vapour at 183° ...	27·4	29·4	45·3	50·1	52·5	54·0
1 hour " "	26·3	30·0	47·3	52·2	55·0	56·6
3 hours " "	24·9	32·8	50·9	57·3	60·0	62·2
7 " " "	23·7	34·3	54·7	62·3	66·0	68·0
13 " " "	22·8	35·0	58·0	66·4	70·8	72·0
1 minute in molten lead at 330° ...	18·9	33·0	64·3	80·7	88·6	92·2
1 hour " "	17·4	30·0	65·3	84·6	95·0	100·8
Heated to glowing ...	14·5	6·0	27·8	47·5	63·3	74·0

A comparison of the several curves and tables shows that each kind of wire, and indeed each wire, behaves differently. The several curves nowhere are concurrent. The thicker wires show a greater power of being magnetised than the thin ones. In the case of the thicker wires there seems to be a relation with the specific gravity. But it is to be remarked that amongst all these individual differences there is a general family likeness in all the curves. Their general course shows clearly that the relative dimensions do not play so important a part as has generally been imagined; certainly they do not show any critical value below which magnets behave differently to what they do above it. The changes are, on the contrary, continuous. In general it appears that the magnets, whether short or long, slightly lose their power of being magnetised when annealed, until they reach a minimum for a certain degree of hardness.

On continuing the annealing the value rises again to a maximum, and then sinks again when the wires are heated to glowing. The thinner and longer the magnets are, the wider apart are the minima and maxima, and the greater is the difference between them. As a practical deduction it seems that with short magnets the glass-hard steel is the best, while with elongated magnets better results would be obtained by using very nearly soft steel.

IDEM—INFLUENCE OF ANNEALING ON THE RETENTIVENESS OF MAGNETS.

(*Annalen der Physik und Chemie, B. XX., H. 4, No. 12a, 1883, p. 662.*)

In all former experiments such temperatures have been made use of as not only altered the magnetism of the bar experimented upon, but also altered the condition of the material itself; and this alteration, which has not been taken account of, vitiates the results arrived at. The authors therefore undertook a very exhaustive research into the question, the methods and instruments being the same as those employed in the work of which a summary is given above.

The first experiments were made with six small bar magnets, which were magnetised by means of a large Funkler's horse-shoe magnet, and then placed alternately ten times in one water bath at 15° C., and in another at 50° C., when a loss of magnetism was noted. To still further test the question, the same magnets were exposed to the action of steam at 100° C. for various intervals of time, from twenty minutes to four hours. The tabulated results show a continuous loss of magnetism as the annealing was continued, the amount remaining after four hours' heating being in each of the six magnets less than half the original amount: the greatest loss occurred during the first forty minutes. If a curve is plotted from the results obtained, it can be seen at once that the curve at first falls very suddenly, then more and more gradually, and finally tends to become parallel to the X axis. The curve obtained is so similar in its course to that obtained in the previous research for the annealing of steel as to suggest at once the idea that the observed decrease in the magnetic force is due to the same cause as the annealing of the glass-hard steel. To test the truth of this conclusion, two steel wires were experimented upon, each 10 cm. long, and weighing, the one 0·417 gr., the other 0·418 gr. The former was magnetised, and then kept in steam continuously for six hours, and observations taken at first every ten minutes, and then every hour, when it was found that the specific magnetism fell from 62·6 to 43·8. The second magnet was treated in the same way, but was remagnetised after each observation. In this case the specific magnetism only decreased from 59·7 to 56·5, but the specific magnetism after each remagnetisation was smaller and smaller each time: its original value was 62·5, and after nine successive heatings and remagnetisations, 59. The specific resistance was determined concurrently, and was found to decrease proportionally to the specific magnetism; and, as the former is a measure of the degree of annealing, it was evident the

continuous decrease of the magnetic moment is dependent upon the progress of the annealing.

The experiments showed, further, that it is indifferent whether a glass-hard steel bar is first annealed in water at 100° C. till it has reached its limiting condition, and then magnetised to saturation, or *vice versa*. The use of annealed instead of glass-hard magnets has the one great advantage, that their sensibility to changes of temperature is much less. The magnet in the glass-hard condition remains with its molecules in a very strained state: annealing diminishes this, so to speak, unnatural state of tension, and gives rise to a condition of greater stability. If a magnet is annealed in steam at 100°, it will lose some 70 per cent. of its original magnetism: if, however, it now be remagnetised and again annealed, it will only lose 5 to 10 per cent. Moreover, such magnets are not affected by blows or mechanical disturbances, as was proved by experiments.

The authors conclude by giving the following practical rules for the production of constant magnets:—

1. Glass-hard steel bars are quite unsuitable.
2. If the steel bar at ordinary temperatures is well hardened, it should be kept in steam for 20 or more hours. The magnet is then in the limiting state of hardness corresponding to 100° C.
3. The bar is then to be magnetised to saturation, and again kept in steam for about 5 hours, when it will have arrived at a stationary condition.

F. BRAUN—SIMPLE AND CONVENIENT METHOD OF CALIBRATING WIRES BY MEANS OF THEIR RESISTANCE.

(*Beiblätter*, *B.* 7, *H.* 10, *No.* 10, 1883, *p.* 776.)

A current, which is maintained constant by means of a rheostat in circuit, traverses the wire to be experimented upon, on which slide two knife-edges at a measurable distance apart—say, 100 millimètres. Wires from these knife-edges lead to a galvanometer, which is thus a shunt to the experimental wire, and the current is arranged so that the galvanometer shows a deflection of 100 divisions. If the total resistance of the shunt circuit is, say, 400 ohms, while the wire has a resistance of 1 ohm, the resistance at the points of contact of the knife-edges and the wire may be neglected. The two knife-edges, at the constant distance from each other of 100 millimètres, are then slid along the wire, and the change in deflection of the galvanometer noted, or the deflection may be maintained by altering the distance apart of the contact points. In either way we can arrive at a determination of the homogeneity of the wire and its regularity as regards diameter.

IDEM—CONVENIENT FORM OF MIRROR GALVANOMETER.

(*Beiblätter*, *B.* 7, *H.* 10, *No.* 10, 1883, *p.* 780.)

The description relates to several improvements introduced into Wiedemann's form of instrument. The readily interchangeable coils slide on a brass

tube provided with a guide rod and rack. One of Siemens' bell magnets is used in a copper damping case, which is divided vertically. By separating the two halves by a piece of cardboard, or by raising the magnet out of the case, it can be made less dead-beat. The needle can be set to zero by altering the position of an iron ring which surrounds the coils as near as possible to the magnet.

Dr. H. HAMMERL—COPPER VOLTAMETER.

(*Elektrotechnische Zeitschrift, B.* 4, *H.* 12, *Dec.*, 1883, *p.* 501.)

As is well known, the electrolysis of a solution of copper sulphate proceeds in an abnormal way when the current-density is too great, and secondary actions take place. The author has undertaken a series of experiments, in which two copper voltameters were connected up in series in circuit with a tangent galvanometer and a source of electricity. One voltameter was retained unaltered throughout the experiments, while in the other continual changes were made in the substance of the electrodes, in their size and distance apart, and the temperature and state of rest of the copper sulphate solution; comparisons were then made by careful weighings of the plates in the two voltameters. After the plates were withdrawn from the solution, they were well washed with distilled water, dried between folds of filter-paper, and finally placed under the receiver of an air-pump, which was exhausted several times, air thoroughly dried being admitted each time to replace that pumped out. The plates thus dried could be slightly warmed over a flame; though, had this been done when they were wet, more or less oxidation of the surface would have taken place.

From such experiments the author assured himself that it made no difference whether platinum plates were used bare or with a layer of copper deposited on them. The weights of the deposit in the two cells only differed by about 0·02 per cent. The tangent galvanometer showed that the current fell off within a few minutes. This is due to the using up of the copper sulphate in the solution at the kathode; as soon as this happens, polarisation begins and the current diminishes. This action may in great measure be prevented by brisk stirring of the liquid. It was not possible to keep the liquid in constant agitation by boiling it, as the copper begins to oxidise at 90° C.

In order to test the effect on the deposit of changes in the current-density, two identical voltameters were used. One was unaltered, while more and more of the solution was taken out of the other, thus increasing the current-density. The resistance in the cell of course increased, but this was compensated by plugging out resistances in the circuit, so that the current remained the same in all experiments. So long as the current-density is not too great, the tangent galvanometer shows a constant deflection, and the weights of copper deposited in the two cells are equal. On increasing the density, the deflection at first increases, owing to the rise of temperature of the solution

in the voltameter II., which has been diminished, and then falls off; and the area of the plate in II. is so small that a good deal of copper is deposited on the edges and corners, which leads to loss, and a difference in the weights of the deposits I. and II. On increasing still further the density in II., a slight polarisation is set up, which becomes greater as the current-density increases. When once polarisation has begun, the weights of the deposits differ from each other, and from what they should be to correspond with the reading of the tangent galvanometer. With a very considerable density the copper is no longer deposited pure, but forms a spongy mass of copper hydride. As a result of his experiments, the author finds that in order to be able to take the amount of copper deposited as an exact measure of the current, this should not ever exceed 7 ampères per square décimètre of the surface of the kathode (say, 65 ampères per square foot). Besides the current-density, the form of the electrodes has an influence on the deposit, since this has a tendency to form at the edges and corners. Long, narrow plates should be avoided, and preference given to those as nearly square as may be. The less the distance between the two plates, the sooner is polarisation set up. The distance in most experiments was 2·6 cm., and it was found that for ·35 cm. polarisation occurred when the current had reached 4·19 ampères per square décimètre.

L. WEBER.—CHANGES IN THE RESISTANCE OF A BARE, FREELY-SUSPENDED, WIRE DUE TO THE PASSAGE OF STRONG CURRENTS.

(*Elektrotechnishe Zeitschrift, B. 4, H. 12, Dec.* 1883, *p.* 519.)

In the experiments undertaken to determine the above changes, a current of electricity, the strength of which could be varied at will by plugging out more or less resistance, was made to traverse for ten or twelve minutes the wire to be tested. The strength of this current was measured on a dead-beat mirror galvanometer with a bell magnet, shunted by a thick copper wire. At the end of the ten minutes the current from the dynamo machine was switched off with one hand, while by means of a commutator worked by the other hand, the now heated wire was connected up to a bridge on Siemens' pattern. By continued repetitions of this process, the resistance of the wire was determined within 0·005 ohms. The change of resistance being known, the heating of the wires was calculated from the formula

$$v - u = \frac{r_2 - r_1}{r_1 \times \varpi},$$

where ϖ is the specific conductivity of the wire. When a state of equilibrium has been reached in the wire, we must have

$$(v - u)\, h = \frac{I^2\, S}{r^3} \times 0{\cdot}069423,$$

in which s is the specific resistance, r the radius of the wire, and h the number of calories given out in one minute by unit of surface of the wire for a difference of temperature $(v - u) = 1^\circ$ C.

From the tables given the following are selected:—

I.—*Chemically Pure Copper Wire.*

I.	Δ Increase of Resistance.	$v - u$.	h.
Ampères.	Per cent.	Degrees C.	
5·12	2·5	5·8	0·0505
5·94	3·0	7·1	
7 7	4·7	11·0	
9·44	6·8	15·8	
5·07	2·6	6·0	0·0458
5·91	3·3	7·8	
7·63	5·1	12·0	
9·41	7·7	17·9	
4·99	2·1	4·9	0·0519
5·91	2·9	6·9	
7·57	4·7	10·9	
9·39	6·8	15·9	

II.—*Galvanised Iron Wire.*

I.	Δ Increase of Resistance.	$v - u$.	h.
Ampères.	Per cent.	Degrees C.	
4·41	0·4	1·2	0·0353
6·13	1·2	3·5	
8·62	2·8	8·2	
4·11	1·1	3·2	0·0191
6·11	1·8	5·3	
8·51	3·2	9·1	
4·05	0·6	1·7	0·0272
6·71	1·3	3·8	
8·65	2·8	7·9	
9·96	3·5	10·0	

Dr. O. FRÖLICH—ELECTRICAL MEASUREMENT OF THE SUN'S HEAT.

(*Elektrotechnische Zeitschrift, B.* 5, *H.* 1, *Jan.* 1884, *p.* 3.)

It is a well-known law of physics, that in every system of bodies upon which

constant or periodic forces are at work, the course of the physical phenomena after a long time must become constant or periodic, no matter in what condition the system may formerly have been. From which we may conclude, that if the sources of heat acting on the earth are constant or periodic, and if, further, nothing changes in the rate of motion and nature of the earth, the meteorological phenomena must also follow a periodic force. Since the heat radiated to us from the stars and the heat which reaches the surface from the interior of the earth do not change, we must look for the cause of meteorological changes in the changes of the sun's heat.

The chief object of this research is to measure the amount of heat which reaches the earth's surface, allowing for the absorption by the atmosphere. The instrument used was a thermopile in connection with a delicate astatic galvanometer,—both made by Siemens and Halske,—which the author prefers to the more recent instruments, such as Langley's bolometer. The great difficulty of the question lies in the selection of a standard unit for comparison.

The first standard tried was a platinum sheet kept at a white heat by means of a benzine gas lamp, which gave results within 3 or 4 per cent.: with these, however, the author was not satisfied. He then tried with greater success an incandescence lamp as standard, in which the heat radiated can be measured by the electrical work done in the fibre. It was found, however, that the ratio between the radiation and the energy expended decreased, owing, no doubt, to the gradual disintegration of the carbon filament.

Finally, the dark heat rays radiated from two Leslie's cubes were adopted as standard. One of these cubes had its sides covered with dull black varnish, the other with white chalk: so long, then, as the ratio of the radiations from these two surfaces remained the same, it might be safely concluded that the amount of radiation from each was unchanged.

The first experiments were made in September, 1879, on the Faulhorn, at an altitude of about 9,270 feet. It was only possible to take observations on one day during a three weeks' stay, and the results obtained could not be considered very accurate. They tended, however, to show that the absorption by the atmosphere is much less on high mountains than in the plains below. Further observations were made in 1881-1882 in the environs of Berlin, and from the curves plotted it appears that the logarithm of the sun's heat is a linear function of the length of path of the rays.

From measurements made at the top of a tower at the West End of Berlin, in the summer of 1883, it was apparent that a decrease in the heat radiated from the sun had taken place. The course of the curves obtained shows that from the beginning of July to the middle of August there was an increase of about 6 per cent. above the normal, while from the middle of August to the middle of September there was a decrease of about 8 per cent., and then no particular variation up to the middle of October. The main point which is settled is, that changes in the amount of heat radiated by the sun can be observed, and that they afford a means for the measurement of this heat.

F. KOHLRAUSCH—ABSOLUTE MEASURES OF STRONG CURRENTS BY MEANS OF A TANGENT GALVANOMETER, AND DESCRIPTION OF A SPRING GALVANOMETER FOR TECHNICAL USE.

(*Elektrotechnische Zeitschrift*, *B.* 5, *H.* 1, *Jan.*, 1884, *p.* 13.)

In the author's opinion a tangent galvanometer is still the simplest instrument for the measurement of currents, and he considers the limits within which the errors are inappreciable, and the formula for obtaining values in absolute units. An error may be introduced by the instrument not being placed correctly in the magnetic meridian, and this error may amount to 1·7 per cent. for a deflection of 45°, and to 10 per cent. for 80°. It may, however, be reduced very largely by using a commutator, so as to reverse the direction of the current, and taking the mean of the two deflections: if this is done, the error for 80° deflection may be reduced from 10 to 1 per cent. An error of 0·2° in reading the deflection will cause an error in the current of not more than 1 per cent. if the deflections lie between 20° and 70°, but may rise to 4 per cent. by overstepping these limits. Since $\frac{\tan. 72^\circ}{\tan. 18^\circ} = 10$ about, the same galvanometer can be used with exactness in the measurement of currents between the limits 1 and 10.

Weber's formula for the current is

$$I = \frac{R\,H}{2\pi\,n} \tan. \phi,$$

where R = mean radius of ring, H = horizontal earth's force, n = number of turns in ring. If R is measured in centimètres, and H in C.G.S. units, we shall have

$$I = \frac{R\,H}{2\pi\,n} \tan. \phi \text{ absolute units,}$$

or

$$I = 1{\cdot}592 \frac{R\,H}{n} \tan. \phi \text{ ampères.}$$

This formula should be corrected for the length of the needle, l, by introducing the factor $\left(1{\cdot}592 - 0{\cdot}2\,\frac{l^2}{R^2}\right)$ in place of 1·592.

If the length of the needle has the value of 1 : k of the diameter, the deflection has to be increased by a number d (found from the following table), multiplied by $\frac{100}{k^2}$.

Degrees.	Degrees.	Degrees.	Degrees.
ϕ = 5	d = 0·00	ϕ = 50	d = 0·43
„ 10	„ 0·01	„ 55	„ 0·47
„ 15	„ 0·03	„ 60	„ 0·49
„ 20	„ 0·06	„ 65	„ 0·47
„ 25	„ 0·10	„ 70	„ 0·42
„ 30	„ 0·16	„ 75	„ 0·35
„ 35	„ 0·23	„ 80	„ 0·25
„ 40	„ 0·30	„ 85	„ 0·13
„ 45	„ 0·37		

An example may render the calculation more clear:—H = 0·187, R = 15 cm., $n = 1$, $l = 5$ cm., ϕ (right) = 55·5°, ϕ (left) = 55·8°, ϕ (mean) = 55·65° hence substituting in formula

$$I = \left(1{\cdot}592 - 0\,2\frac{l^2}{R^2}\right)\frac{R\,H}{n}\tan.\left(\phi + d\,\frac{100}{k^2}\right)$$

$$= \left\{1{\cdot}592 - 0{\cdot}2\left(\frac{5}{16}\right)^2\right\}\frac{15 \times 0{\cdot}187}{1} \times \tan.\left\{55{\cdot}65 + 0{\cdot}48\left(\frac{100}{36}\right)\right\}$$

$= 6{\cdot}776$ ampères.

By using a shunt (p) of $\frac{1}{9}$ the resistance of the above galvanometer, it is possible to measure from 2 to 200 ampères, which will suffice for most purposes.

The author states the dimensions to be given to this shunt wire, so that it may not be unduly heated by the strong currents used. One ampère passing through a resistance of 1 ohm develops 0·24 calories per second: since the specific heat of copper is 0·095, this quantity of heat will raise the temperature of 1 gramme of copper 2·5° C. A copper wire of d mm. diameter weighs $7 \times d^2 \times g$ grammes per mètre, and has a resistance of $\frac{0{\cdot}023}{d^2}$ ohms per mètre; hence a current of I ampères will develop per second and per mètre the quantity of heat

$$0{\cdot}24 \times I^2 \times \frac{0{\cdot}023}{d^2} = 0{\cdot}0055\left(\frac{I}{d}\right)^2 \text{ calories.}$$

Since this wire weighs $7 \times d^2 \times g$ grammes, and since 1 gramme of copper is heated by 1 calorie through 10·6°, this copper wire will be heated through

$$\frac{10{\cdot}6}{7\,d^2} \times 0{\cdot}0055\left(\frac{I}{d}\right)^2 = 0{\cdot}0083\,\frac{I^2}{d^4} \text{ degrees.}$$

A wire 2 mm. in diameter, with a current of 18 ampères would be heated in one second through about 0·17°, and, if the current were passed continuously, would be raised altogether about 25°. In order that this amount may not be exceeded, he recommends the use as shunt of several wires of 2 mm. diameter each, joined up in multiple arc, so as to give the joint resistance required for the shunt, and so that each wire does not carry more than 18 ampères.

In the latter part of the article, the author describes a simple form of instrument in which the current is measured by the extent to which an iron core is drawn into a coil of wire. On the base of the stand is fixed vertically a coil of the following dimensions:—length, 14 cm.; inside diameter, 2 cm.; outside diameter, 4·5 cm. From the top of a vertical support fixed to the base an iron tube 20 cm. long and 1·4 cm. diameter is suspended by means of a spring, so that it hangs with 4 cm. of its lower end in the coil: the weight of this core is 30 grammes. The instrument can be readily calibrated, and the value of the current in ampères can be marked on the core itself. Naturally, for weak currents the divisions are wide apart, while for stronger currents (the instrument illustrated reads up to 40 ampères) they are crowded together.

F. VON HEFNER ALTENECK—ATTEMPT TO OBTAIN A CONSTANT UNIT OF LIGHT.

(*Elektrotechnische Zeitschrift, B.* 5, *No.* 1, *Jan.*, 1884, *p.* 20.)

In his researches in connection with electric light, the author has been beset by the difficulty which has been felt by so many, viz., the want of a constant unit of light, and he has attempted, and very successfully too, to overcome the difficulty by working with a particular kind of lamp, fed by a particular kind of fuel.

This unit of light is the illuminating power of a freely-burning flame which rises from the square-cut top of a wick saturated with acetate of amyl, the wick being contained in a round tube of German silver of an inside diameter of 8 mm. and an outside diameter of 8·2 mm., the length of the tube being 25 mm. The flame reaches a height of 40 mm. from the top of the burner, and it should be lighted at least ten minutes before being used.

The exact height of the flame is readily fixed by the aid of a sight carried on the top of a rod fitting into the base of the lamp. The wick is formed of strands of ordinary lamp cotton, the threads running about fifteen to twenty to the inch, and enough are taken to fill, without compression, the German silver tube. The top of the wick must be cut quite flat. The amount of fuel in the lamp does not matter, provided all the threads dip into it. To protect the flame from currents of air, it can be surrounded by a wide glass chimney; but, if this is done, allowance must be made for the absorption of the glass.

A very long series of experiments were made with every kind of liquid fuel, and acetate of amyl was finally selected, both because it showed great constancy of light and because it can be readily procured, being sold largely under the name of "pear oil," which is used by perfumers and confectioners. The light is about equal to that of a standard spermaceti candle, with a flame 44 mm. high—*i.e.*, it forms just a mean value to the various determinations made of the standard candle.

A. PERENYI—DIMENSIONS OF CONDUCTORS FOR STRONG CURRENTS.

(*Elektrotechnische Zeitschrift, B.* 5, *Nos.* 1 *and* 2, *Jan. and Feb.*, 1884, *p.* 24 *and p.* 70.)

In laying down electric mains for currents such as can be used for lighting and transmission, the choice of the diameter of the wire to be used is influenced by two things—small capital outlay and low working expenses. The former would lead to the use of wires of small diameter; the latter to the use of very large wires, since the larger the wires, the less the energy wasted as heat in overcoming their resistance. As, therefore, these two grounds for making choice of a wire are directly opposed to each other, some mean value for the diameter should be found which will reduce both capital outlay and working expenses to a minimum.

The solution which has been given to this question is sufficiently well known, but it may be interesting to review somewhat in detail the method by

which the author arrives at his results. He considers first, the amount of heat developed in a wire, and secondly, the most economical size of wire.

From Joule's law we have the quantity of heat developed per second in a conductor

$$Q = \frac{I^2 R}{9{\cdot}81 \times 424} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

This quantity of heat, which is measured in kilogramme-degrees, raises the conductor in one second through T°. If now c is the quantity of heat required to raise one kilogramme of the metal from 0° to 1°, then also

$$Q = c\,\gamma\, q'\, l\, T \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

where γ is the weight of one cubic mètre of the metal, l the length of the conductor, and q' the section in square mètres. If we consider that the amount of heat radiated is small in comparison with the amount developed,—and even if it is not, the only effect on the calculation would be to bring out the diameter of the wire smaller, so that we shall be on the safe side,—neglecting the radiated heat, then, we may equate the above values of Q, or

$$I^2 R = \phi\, l\, q'\, T, \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

$$\text{where } \phi = 9{\cdot}81 \times 424 \times c \times \gamma.$$

In equation (3) we may replace R by $\frac{a'\,l}{q'}$, in which a' is the resistance of the conductor per square mètre of section, and for convenience it is better to express the section q in millimètres. Making these reductions, we shall have

$$I^2\, a'\, l = \phi\, l\, q'^2\, T$$

$$I = q' \sqrt{T \cdot \frac{\phi}{a'}}$$

$$= 10^{-3}\, q \sqrt{T \cdot \frac{\phi}{a}} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

in which now q is expressed in square millimètres, and a is the resistance of the conductor per square millimètre of section.

If in equation (4) $q = 1$, $T = 1$, we have $10^{-3}\sqrt{\frac{\phi}{a}}$ as an expression for the current which will raise a conductor one square millimètre in section through one degree in one second. For copper wire we have the mean values $a = 0{\cdot}02$ ohm, $c = 0{\cdot}095$ calories per kilogramme, $\gamma = 8{,}900$ kilogramme per square metre; hence $\phi = 3{,}517{,}200$ and $10^{-3}\sqrt{\frac{\phi}{a}} = 13{\cdot}262$ ampères; for iron this value, which the author calls the specific current, becomes 5·44 ampères If the conductor consists of a round wire, we may write $\frac{d^2\,\pi}{4}$ for q in (4), and we have then

$$I = 10^{-3}\,\frac{d^2\,\pi}{4}\sqrt{T \cdot \frac{\phi}{a}} \quad \ldots \quad \ldots \quad \ldots \quad (5)$$

or putting $10^{-3}\sqrt{\frac{\phi}{a}}$, the specific current $= S$,

$$I = S\sqrt{T} \cdot \frac{d^2\,\pi}{4} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (6)$$

In any complete circuit we have, by Ohm's law,

$$I = \frac{E}{R + r},$$

and combining this with equation (4),

$$q = \frac{1}{r}\left(\frac{E}{S\sqrt{I}} - a\,l\right) \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (7)$$

Coming now to the question of working expenses, let n be the number of working days in the year, p the hours worked per day, P the price of the metal per kilogramme, H the average cost of 1 H.P. for an hour; then the H.P. lost in the conductor is $\frac{I^2\,a\,l}{9{\cdot}81 \times 75\,q}$, and the value of this loss

$$v_1 = \frac{I^2\,a\,l}{q} \cdot \frac{H\,n\,p}{736}.$$

If we reckon 5 per cent. interest on the outlay for the conductor of $\gamma\ l\ q'$ weight,

$$v_2 = \gamma\,\frac{10^{-6}}{20}\,P\,l\,q.$$

Thus the total yearly loss is $v_1 + v_2$, and this will be a minimum when

$$\frac{\delta\,(v_1 + v_2)}{\delta\,q} = \gamma\,\frac{10^{-6}}{20}\,P\,l - \frac{a}{736}\,\frac{l}{q^2}\,H\,I^2\,n\,p = o$$

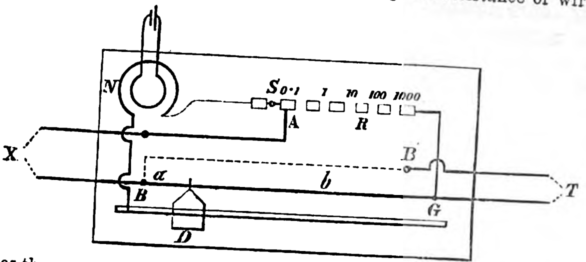
$$\text{Whence } q = 162{\cdot}56\ I\sqrt{\frac{a}{\gamma} \cdot \frac{H}{P}\,n\,p}.$$

Taking H as being equal to 291 marks, the author deduces that the area in square centimètres of the conductor should be 3½ per cent. of the current measured in ampéres. Assuming Sir Wm. Thomson's values of H and P, which, however, the author considers too low, the area in square centimétres would work out to be 2 per cent. of the number of ampéres.

F. KOHLRAUSCH—MEASUREMENT OF THE RESISTANCE OF LIQUID CONDUCTORS, AND A UNIVERSAL RESISTANCE MEASURER.

The method is founded on the balance principle, but in place of a battery the secondary of an induction coil is used, and in place of a galvanometer a telephone. When no sound is heard in this latter, the several arms of the bridge balance each other. The diagram shows the arrangement and connections. N is the induction coil (a so-called Neeff's hammer), making about 150 contacts per minute, and having its primary excited by one Bunsen or bichromate cell, or by two Daniells. The liquid resistance, which may be that of a galvanic cell, is inserted at X. It should be noted that this method of alternating currents is specially adapted for measuring the internal resistance, since it gets rid of the polarisation. BC is a wire of German silver, 25 cm. long and 0·3 mm. thick, with which the pointer D makes contact. This pointer moves over a scale, not shown, which is so graduated as to give at once the ratio $a : b$, the number read off has therefore only to be multiplied by the

resistance, R, unplugged, to obtain the resistance of the conductor measured. The method can of course be used for measuring the resistance of wires so



long as they are straight: if in coils the result would be vitiated by the extra currents set up.

LIST OF OTHER ARTICLES.

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(Elektrotechnische Zeitschrift, Bd. 4, *No.* 12, *December,* 1883.)

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JOURNAL

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The One Hundred and Thirty-fourth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 24th April, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed, and the names of new candidates were announced and suspended.

Donations to the Library were announced as having been received from G. J. Symons, F.R.S.; a collection of works from the library of the late General Sir E. C. Sabine; C. S. Jones, Chicago; Iron and Steel Institute; Radcliffe Library, Oxford; J. P. Tawse; Institution of Civil Engineers; Dr. Zetzsche; La Société Française de Physique; to whom the meeting expressed a hearty vote of thanks for their presents.

The following paper was then read :—

ON THE RELATION WHICH SHOULD SUBSIST BETWEEN A CURRENT OF ELECTRICITY AND THE CONDUCTOR EMPLOYED TO CONVEY IT.

By THOMAS H. BLAKESLEY.

At a meeting of the Society of Telegraph-Engineers and Electricians on December 13th, 1883, I put forward in the course of the discussion, and in a general form, the grounds for dissenting from the views generally held, or at least expressed, as to the

applicability of Sir Wm. Thomson's law of economy in conductors to cases to which that philosopher would certainly never have applied it. Neither time nor the power of memory allowed me to do more than treat the subject most generally; but the importance as regards the outlay of money and loss of energy can only be thoroughly appreciated by giving values to symbols and going practically into the results. This I propose doing in the present communication.

The dogmatic forms given to the law of economy are the two following:—

1. The area of cross-section of the conductor is proportional to the current it has to carry.

2. The interest on the whole outlay in installing the conductor is equal to the value of the energy absorbed.

These two forms are different ways of expressing the same idea, and the one is true or untrue in the cases when the other is true or untrue, as will be shown.

Let C be the current required to be carried by.

„ r = the resistance of 100 yards of conductor, of cross-section 1 square inch.

„ y = the cost of one horse-power electric maintained for one year.

„ t = the proportion of entire time employed.

„ x = rate of interest on money laid out in installing the conductor, together with sinking fund required annually to maintain the installation in working order, per centum of the original cost.

„ A = the area of cross-section of conductor in square inches.

Further, let the whole expense of the conductor and its installation be dependent on its cross-sectional area in a way represented by a series of terms involving any powers of that area, not omitting the zero power which will occur in a constant term, as follows:—

$$a + b\,\mathrm{A} + c\,\mathrm{A}^m + d\,\mathrm{A}^n + \text{etc.}$$

Here a is a constant term; $b\,\mathrm{A}$ is a term varying as the cross-section of conductor; $c\,\mathrm{A}^m$, $d\,\mathrm{A}^n$, etc., are any other terms involving any other powers of A.

The value of the interest and depreciation on this sum is equal to

$$\frac{x}{100}\left\{a + b\,A + c\,A^m + d\,A^n \text{ etc.}\right\}$$

The power expended is $\frac{C^2 r}{A}$ in watts. But one watt costs $\frac{y}{746}$ when maintained for a year; therefore the power employed would cost $\frac{C^2 r}{A} \cdot \frac{y}{746}$, if employed during the whole year. But when the fraction of the year the current flows is only t, the cost per annum will be $\frac{C^2 r}{A} \cdot \frac{y}{746} \cdot t$. Therefore the total cost in energy absorbed and conductor installed and maintained per annum is

$$\frac{C^2 r\,y\,t}{A\,746} + \frac{x}{100}\left\{a + b\,A + c\,A^m + d\,A^n \cdots\right\}$$

Now this form generally admits of a minimum, found by differentiating it with regard to A, and equating the result to zero. Thus

$$-\frac{C^2 r\,y\,t}{A^2\,746} + \frac{x}{100}\left\{b + c\,m\,A^{m-1} + d\,n\,A^{n-1} + \cdots\right\} = o.$$

Multiplying through by the quantity A, and transferring terms,

$$\frac{C^2 r\,y\,t}{A\,746} = \frac{x}{100}\left\{b\,A + c\,m\,A^m + d\,n\,A^n + \cdots\right\} \quad \ldots\ldots\ldots\ldots a.$$

$$= \frac{x}{100}\left\{a + b\,A + c\,A^m + d\,A^n + \cdots\right\}$$

$$+ \frac{x}{100}\left\{-a + \overline{m-1}\,c\,A^m + \overline{n-1}\,d\,A^n + \cdots\right\} \ldots \beta.$$

The term on the left is equal to the cost of the energy absorbed, and the first series on the right is the annual cost of installing and maintaining the conductor, and these two cannot be equal to one another except when the second series on the right is equal to zero.

This occurs when $a = 0$, $m = 1$, $n = 1$, etc., *i.e.*, when there is no absolute term as a, and no power of A involved in the conductor but the first power; and when this is the case the equation α shows that C^2 varies with A^2, for then $\frac{C^2}{A^2}$ is constant or $\frac{C}{A}$ is constant, and the area is proportional to the current it has to carry. But when the first power is involved alone in the expense of the conductor, the meaning is that the cost is pro-

portional to the area. Now I propose to show, first, that in an insulated conductor in the form of a cable, the expense of the conductor is not proportional to the area of the cross section; and, secondly, to show that the price of the cable can be formulated very closely in terms of the cross-section. And obviously, if we can do this satisfactorily, we have no difficulty in finding at once the economical current for a section by the foregoing rules.

To carry out these points I beg to draw attention to the following table, compiled from the price-list of Messrs. Siemens Brothers, of Charlton:—

Yards giving $\frac{1}{10}$ ohm resistance.	No. of wires.	Area. A	Virtual diameter $= \sqrt{\frac{4A}{\pi}}$ d	a.	$91.783 \times A$.	b.	$\overline{b - a}$.	$.23 + 11.4d$.	$.23 + 11.4d + 91.783A$.	c.	$\overline{c - a}$.	$1.14 + 13d$.	$1.14 + 13d + 91.783A$.
100	7	·026	·182	2·5	2·39	4·75	2·25	2·80	4·69	6·	3·5	3·5	5·89
200	14	·052	·257	4·75	4·77	8·	3·25	3·16	7·93	9·5	4·75	4·48	9·25
275	19	·0715	·302	6·5	6·56	10·	3·5	3·67	10·23	11·5	5·	5·07	11·63
535	37	·1891	·421	12·5	12·77	18·	5·5	5·13	17·90	19·5	7·	6·61	19·38
880	61	·2288	·540	21·	21·	27·	6·	6·38	27·38	29·	8·	8·16	29·16

d.	$\overline{d - a}$.	$.4 + 8.934d + 21.186A$.	$.4 + 8.934d + 112.969A$.	e.	$\overline{e - a}$.	$1.66 + 11.181d + 18.895A$.	$1.66 + 11.181d + 110.678A$.	f.	$\overline{f - a}$.	$3.25 + 12.143d + 22.069A$.	$3.25 + 12.143d + 113.852A$.
5·	2·5	2·57	4·96	6·5	4·	4·19	6·58	8·5	6·	6·08	8·42
8·75	4·	3·80	8·57	10·50	5·75	5·52	10·29	12·50	7·75	7·52	12·29
10·75	4·25	4·61	11·17	12·50	6·	6·38	12·94	14·75	8·25	8·49	15·05
19·5	7·	7·11	19·88	21·50	9·	8·99	21·76	24·	11·5	11·43	24·20
31·	10·	10·07	31·07	33·	12·	12·02	33·02	36·	15·	14·85	35·85

Each horizontal line refers to the same conductor, and the first column gives the number of yards making up the tenth of an ohm. These numbers are used as denominations for the particular conductors, and are of course, assuming the wires are always of the same conductivity, proportional to the cross-section of the conductor.

The second column gives the number of wires in each conductor.

The third gives the area of cross-section in square inches, called A.

The fourth gives the vertical diameter, called d, deduced from the equation $d = \sqrt{\frac{4\text{ A}}{\pi}}$.

The fifth column gives the price of the bare conductor from the price list. It is marked a. The price is in pounds sterling per 100 yards run.

The next gives the result of applying a linear equation in A, viz., 91·783 A, most nearly giving the actual price.

Then follow five series of four columns each.

Each series corresponds to a specification of the covering of the conductor, as follows:—

b. Insulated with india-rubber and tape.
c. Insulated with india-rubber, jute, and tape.
d. Insulated with gutta percha.
e. Insulated with gutta percha, jute, and tape.
f. Insulated with gutta percha, jute, and tape, and iron sheathed.

The first column of each series gives the price from the price-list of the covered conductor in pounds sterling per 100 yards run.

The second column gives the difference between the preceding column and the price of the bare conductor, that is to say, the price of the insulation.

The third column gives the price of this as deduced from the formula found to give the real prices most approximately.

The fourth column gives the results of the formula of insulation combined with that of the bare conductor.

I have attempted only to find linear equations in d and A to express these prices. Greater approximation could be obtained by more complex formulæ, but even with those I have employed, an inspection will show very rarely, I think, a deviation of as much as five per cent. But even this is not to be found in the cases where the total sum is considered.

Now, whether we look at the formula or at the figures taken from the price list, it is clear that the cost of insulation largely deviates from proportionality to the cross-section of the conductors. Consider, for instance, the 1st and 5th row of the column marked $\overline{c - a}$, the figures being 3·5 and 8 respectively. The conductors are in cross-section as 1 to 8·8, and if the insulation expense were assumed to be proportional to the conductors, taking the 3·5 as the basis, we ought to find the expense of the insulation of No. 880 in this case, 3·5 × 8·8, or 30·8 pounds instead of only 8 pounds, which it is. Similarly with all the other insulation prices. Moreover these insulation prices are not small compared with those of the conductors bare. In many cases they are greatly in excess. According to specification f, it costs six pounds to insulate a conductor costing only £2 10s. 0d.

If we accept the formula as a representation of the price of a cable, it is easy in any particular specification to give the value of the economical current for any required cross-section, and by constructing a curve of values to perform the opposite problem, *i.e.*, to find the proper cross-section for a given current, the other necessary data being of course also given. Or a table may be easily calculated, as in the following cases.

As an example, the following table gives the values of the currents corresponding to certain areas of cross-section under the following data. The specification is that corresponding to e—insulation of gutta percha, jute, and tape.

The conductivity of the copper is $\frac{95}{100}$ of copper giving 1 ohm in 25 miles of 1″ section.

The time of working is 8 in the 24, or $t = \frac{1}{3}$.

The cost of an electrical horse-power maintained for a year is £10.

The rate of interest and depreciation together is 7·5 per cent.

Area in square inches.	Economical Current in ampères.	Economical Intensity or Current ÷ Area.
·01	11	1103
·04	40	999
·09	86·5	961
·16	150·7	942
·25	232	930
·36	332	922
·49	448·8	916
1·00	906	906

If the working hours were 12 in 24, instead of 8 in 24, the numbers in the 2nd and 3rd columns of the table would have to be reduced in the proportion of $\sqrt{3} : \sqrt{2}$, *i.e.*, multiplied by the fraction ·816.

The formula corresponding to the above case is

$$\frac{C^2}{A^2} = 44259{\cdot}2 \left\{\frac{1}{\sqrt{A}} + 17{\cdot}545\right\}.$$

The following table relates to specification *b*, but otherwise the same data are used :—

Area in square inches.	Current in ampères.	Intensity or Current ÷ Area.
·01	10·5	1046
·04	37·28	932
·09	80·19	891
·16	139·2	870
·25	214·25	857
·36	305·28	848
·49	412·58	842
1·00	830	830

The formula is

$$\frac{C^2}{A^2} = 45126{\cdot}1\left\{\frac{1}{\sqrt{A}} + 14{\cdot}27\right\}.$$

Similarly tables and curves can be formed whenever it is possible to formulate the price of the cable.

It is hoped that these cases have sufficiently indicated the method to be followed generally.

Here it may be well to reflect what a great advantage it would be for the general purposes of design if manufacturers were to make a practice of supplying prices in a formulated shape. It is presumed that they all have their modes of calculation, and it seems easy to throw the expression of their results into an algebraical form.

The above results have been deduced from a single price list, and it may be the prices are somewhat high. No allowance, too, has been made for discount, which would have the effect of diminishing the intensity of the economical current in half the ratio of the discount. But enough has perhaps been said to show the fallacy of the ordinary modes of quoting the economical law of Sir William Thomson, and assigning a fixed value to the current density.

The PRESIDENT reminded the meeting that the discussion about to commence was upon the paper read by Professor G. Forbes at the previous meeting, as well as that just read by Mr. Blakesley. Copies of both papers had been distributed, and no doubt, on such an important matter for the future of electric lighting, would give rise to a useful and interesting discussion.

Mr. W. H. PREECE: Mr. President and gentlemen,—I have very few words to say on Mr. Blakesley's paper, which appears to be directed, not against the truth of Sir William Thomson's law, but against some supposed fallacies in the ordinary mode of quoting that law; and I think it will be quite enough for those who have quoted that law incorrectly to answer Mr. Blakesley.

My remarks will be directed to the more important paper given by Professor Forbes, which has certainly added very considerably to our knowledge of heat in conductors, and has directed a considerable amount of attention to a very important subject.

Now, in Professor Forbes's paper, as one might have anticipated from such an author, his physics are unimpeachable; but I am sorry to say that before I end I shall have to call into question what I may call his "practics."

I will take his physics first; and as he has been good enough,

in his paper, to allude to some of my efforts to solve this question, I may at once express my thanks to him for the encouragement that I have received in carrying them on. It was entirely owing to him that I brought before the Royal Society the series of experiments to which he referred, and which have since been published. Since the year 1878 I have been working uninterruptedly at this question; but it is one thing to work, and it is quite another thing to write papers. Very busy men like myself, though very fond of experimenting in order to solve certain questions that arise, have not time always to put those experiments into readable form. Now the whole subject of the relations between heat, currents, and conductors was really threshed out as long ago as 1840 and 1842, by Dr. Joule; though in those days Joule himself little anticipated the advance that was going to be made in the practical applications of electricity. In parenthesis, I should like to call the attention of the Society to a volume that has just been published by the Physical Society, containing nearly all the papers that were written and printed by Joule. During the past ten years I do not think there has been any more interesting, instructive, and valuable acquisition to the electrician's library than these papers that have been collected into a very useful form by the Physical Society. The connection that exists between currents and heat have occupied my attention, as I said, since, I think, the year 1878. With regard to the influence of heat on batteries, I have published no less than three papers bearing on this matter; but it was in particular the effect of currents on lightning protectors that led to the present enquiry. I found it very necessary to determine the size of a fine protecting wire that would, by fusing, protect a submarine cable from lightning, and yet not be damaged by working currents. The strongest possible current that could be put into a working telegraph line is 500 milliampères with the batteries at present employed. Now the current that is induced, or produced, by direct discharge through a telegraph line, very often acquires 40, 50, or even more ampères, so that between 500 milliampères, the greatest possible telegraphic current, and the current from atmospheric electricity, we wanted to have a little margin where

working currents would not damage the protector, and lightning currents could do no harm to the cable. So a series of experiments was started by me, which determined the size of wires that could be fused by different currents, and it appeared that, taking wires of half a mil—"mil" is a very awkward term; I do not know who is the author of it as applied to a thousandth of an inch, but it is a very awkward term, and is constantly confusing us in relation to millimètres; however, it has been introduced, and we must accept it. Half a mil is half a thousandth of an inch, and taking half a mil, three-quarters of a mil, one, two, three mils, and so on, I found that the currents required to fuse the wire commenced by 277 ampères, and went up in the same ratio as the diameters of the wires. This rather bothered me, because it was contrary to my idea of the law, and it led to the enquiries to which I shall refer. But another curious result that came out of those experiments was, that while the currents required to fuse small wires were very small, the rapidity with which small wires assumed a temperature and lost it by emission was so marked that I thought it possible that wires would indicate the presence of heat, even when minute telephonic currents were used. I took a very small platinum wire, fixed it to the centre of a disc, passed telephonic currents through it, and, to my surprise and delight, I found that the heat generated by those small telephonic currents was sufficient to elongate the wire and to contract it so quickly that sounds and speech were reproduced. By the mere passage of currents through a fine platinum wire, the heating and cooling were so rapid that speaking was possible, and by that means I brought out the thermo-telephone, which was, I think, exhibited here. This illustrated the high emissivity of small wires. Well, this led on to the further question of the heating power of currents; and about that time there was a very able series of papers printed in the *Electrical Review*, on "Lighting by Incandescence," where the whole subject of heat in conductors was very ably dealt with. Also at the meeting of the British Association at Southampton, I think, Professor Forbes brought forward a paper showing that that law was not, as many people imagined, the law of squares, nor, as I found, a law by which the current varied directly as the

diameter, nor a law that followed, as it should have done, the diameter into the square root of the diameter, but, as Professor Forbes has expressed it——

Professor FORBES: I stated that the current was not in proportion to the square of the diameter, as some supposed, nor in proportion to the power three by two, as the calculation had given, but more nearly in proportion to the diameter.

Mr. PREECE: That is exactly what I wanted to say. Now, following that, and at Professor Forbes's request, I published before the Royal Society a long series of experiments on copper, Swedish and wrought iron, German silver, and on platinum. I took various diameters of all those wires and applied various currents to them. Then the difficulty presented itself of how was it possible, in testing wires, to seize upon any constant temperature, any fiducial point. In some of the earlier experiments we used the expansion of the wire to indicate its temperature. Draper used that in the first instance, but that became very uncertain when high temperatures were reached. The boiling point was an extremely difficult thing to get at with any degree of accuracy; but it has occurred to me that the point of red heat, where a wire first became visibly luminous, was a very good fiducial point. It had been pointed out by Draper that this point of luminosity was attained by all substances, without exception, at precisely the same temperature, so that whatever substance be taken and raised, by any means, to a temperature which was 525° C., then that object became luminous with the first dull red rays that attack the optic nerves. I took that point, and also the fusing point, but all the experiments enumerated in the paper referred to were on the dull red point. But my friend Mr. Trotter took my results, as well as others, and has produced the accompanying diagram. That diagram indicates, by the first two curves on the left-hand side, what Mr. Trotter has called "Preece's fusing point," and "Preece's dull red heat." However, the first curve indicates the current that will fuse any wire whose diameter is given upon the lower line. The diameters of the wires are indicated on the abscissa in thousandths of an inch, millimètres, and the new Board of Trade gauge. We

could only go to 80 mils, because, if we went beyond, the diagram would be too large to be of much use. The ordinates show the current in ampères, and the isothermal curves are also shown. Now if, for example, you take a wire of 60 mils diameter, and you want to know the current that will raise it 10 degrees, you find where those two lines cross each other, and at the junction you see the current is shown, 7 ampères. Supposing you take 20 degrees and 70 mils, there you find the current 13 ampères. If you want to find the dull red heat, it is arrived at in the same way. Also the fusing point, for if you want to find what current will fuse a wire of 20 mils, on following the two lines to their point of crossing you see the current is about 12½ ampères. All the curves are drawn to indicate the facts I have mentioned upon the different formulæ that have been proposed by different authors. The red line is the Board of Trade law, following the curve of areas, to depict which the diagram is too small. Here you have a curve drawn to illustrate a formula produced by Messrs. Clark, Forde, and Taylor; and also a formula by Mr. Cowling Welch, that is very closely allied to it. Here is a curve, indicating a safe current, that was published in the *Electrician* the week before last by Mr. Geipel. Here we have a curve showing the safe current proposed by Capt. Cardew of the Royal Engineers; and then we have imaginary curves showing how wires can be worked up to any temperature you like. There are several lessons to be learned from these curves, but I will ask Mr. Trotter himself presently to explain the little table on the left-hand side, and one or two points connected with the diagram. All the curves shown go to support Professor Forbes in the conclusion in his paper, and they also indicate one great fact, and that is, that at the present moment we have no recognised law and no recognised formula that electricians and engineers can work to so as to arrange their conductors to be within the proper safe limits. Now Professor Forbes has brought out, not only the striking fact that the proposal to arrange conductors according to some fixed number of ampères per square inch is wrong, but he has brought out the far more important fact, and a novel and very striking one, that the heat generated in covered

wires is really less than that generated in open wires of the dimensions that we usually meet with. I think that he has done good service, not only in attracting attention to the mistakes that have been made, but in also bringing the new fact before us in the way he has done. He might have added one point that is not, I think, generally known, that the heat generated by alternate currents is greater than that generated by direct and uniform currents; so that, however true the law may be for direct steady currents, it is untrue for alternate currents. Now then, Professor Forbes dealt solely with the question of safety, but he did not touch upon the question of economy, and it is here where I am going to join issue with him on what I call the "practics" of his paper. Safety is one thing; economy of working is quite another thing. You cannot carry a current through a conductor without generating heat; you cannot generate heat without wasting energy. As a rule, energy wasted is unseen, it is unfelt, and we do not know that the energy is wasted until we have to pay the coal bill. Mr. Edison designed and carried out in New York a very extensive system of electric lighting, and there, to his surprise, I am told (I cannot speak from authority), he found, when the system was finished, that he could only get three sixteen-candle lamps per horse-power, instead of the eight that he anticipated. The five-eighths of the current that was lost was wasted as energy, and lost as horse-power in the conductor. The waste in a conductor depends solely upon the strength of the current and on the resistance of the conductor. The temperature of the conductor, the subject which Professor Forbes has principally dwelt upon, depends on the surface of emission, and therefore on the diameter. Hence the waste depends on the length of the conductor, while the temperature is independent of the length. He proposes that we shall provide for a current of 70,000 ampères by using a flat band of copper as the conductor, whose thickness is one centimètre, and whose breadth is 28 mètres. Now, two such conductors must necessarily be dealt with, for in every system involving such a current a positive and a negative wire must be employed, because the use of the earth is entirely objected to at the present day. Hence we have to

take into consideration two conductors, each 2,800 centimètres broad and one centimètre thick. I do not know whether Professor Forbes has calculated out the cost of such a conductor, but it appears that, at the present price of copper, each single conductor would cost £354,000 per mile.

Professor FORBES: That is about right.

Mr. PREECE: So that two such conductors would cost £708,000 per mile. That is for the copper alone. Then you have to add to that the cost of the dielectric,—and our experience tells us that the cost of a dielectric always exceeds the cost of the copper, but we will take it to be the cost of copper,—and that means that the conductors for such an electric light system must cost over £1,500,000 per mile. But that is not all, nor nearly all. The energy wasted in such a conductor is as the square of the current multiplied by the resistance, and the square of 70,000 ampères multiplied by resistance and reduced to horse-power comes to this, that in each mile of each conductor of such a system 6,570 horse-power would be wasted; or, if you put that into money value, it means that your conductor will waste somewhere about £65,000 per annum per mile. Well, it costs per mile £1,500,000, and it wastes £130,000 per annum. I have not done. Professor Forbes said that, in opposition to myself, who maintained that electric light leads should be of pure copper, they should be of iron. Very well, let us allow Professor Forbes to have his own way, and to make the conductor of iron, what is the consequence? If the conductor be made of iron, it will waste 40,000 horse-power per mile, which would cost £800,000 per annum.

Professor FORBES: The iron conductor would be six times as large, instead of the same diameter as the copper.

Mr. PREECE: But they would not cost less. I have not taken iron into calculation, and will therefore adhere to the copper conductor, and the figures I have given of the cost of the copper and the cost of energy wasted represent figures which must be good news for gas people, if we were compelled to carry out a system of electric lighting on such lines. But I do not think we shall have to carry out electric lighting on such lines. I think the system such as that indicated in the Provisional Orders of the

Board of Trade must be looked upon as a system of the past. I do not think it is practicable or possible, with the figures before us, to light up large areas on any such plan as that which has been conceived. It has been departed from in America, it has been departed from in suggestions here; and the day has come when, if electric currents are to be distributed for economical purposes, for lighting or other purposes, they are not to be distributed by enormous conductors swallowing up capital and ruining everybody, but they are to be smaller conductors in which high tension currents can be utilised and distributed. It may be by secondary batteries, when secondary batteries are made practical. It may be by secondary generators, such as we have seen attempted on the Metropolitan Railway, though I do not know that that method has reached its practical stage yet. However, I feel quite satisfied with this, that my figures show that the distribution of electricity for lighting purposes by thick conductors, by the parallel system, by such a system as that which has been proposed in my own case, is, I am afraid, out of the question, because it is financially impracticable. However, as I said at first, although I differ with Professor Forbes in his practics (I hope he will prove me wrong, as I am always glad to be contradicted or corrected when I am wrong), I agree with him most thoroughly in his physics. I cannot agree with him in his proposal that electric light systems shall be provided with conductors consisting of two bands 2,800 centimètres broad. Indeed, I do not know where are the streets or the roads existing at the present time where they could be laid down.

Mr. Alexander P. Trotter: This table of curves (Fig. A) was made, in the first instance, as an extension of the diagram given in Mr. Preece's paper for the Royal Society on the "Heating Effects of Electric Currents," with a view of finding what current would fuse, or raise to dull heat, wires of various diameters.

I had already attempted to draw up a complete table of curves, more complete than this one, working from the formula given by Mr. R. E. Day in the valuable book referred to by Professor Forbes; but there is no information even in Professor

Everett's "Units" on the emissivity for high temperatures. As far as 81° C. the table given in the paper made the completion of the graphical table of curves a matter of minutes instead of hours.

The ordinates are ampères, the abscissæ are diameters, in mils. The dotted lines are isothermals for different temperatures above the air (20° C.) interpolated from Professor Forbes's figures. The full lines show the various "safe currents" according to Captain P. Cardew, R.E., Mr. Geipel, Mr. Cowling Welch, and Messrs. Clark, Forde, and Taylor. These all follow the same law, and are practically statements of opinions as to the degree of temperature which is considered "safe."

It should be noted that this table (Fig. A), extending only to 80 mils, is more theoretical than practical; for, as Professor Forbes has shown, for small wires and small currents the limiting consideration is economy; but the law holds good for heavier currents, where considerations of economy would be satisfied by a wire which would be fused by the current that it was designed to carry. Fig. B gives an extension of Fig. A on a smaller scale, and under conditions where the limitation is safety.

When any formula for the proper current is plotted on this table, and the curve is found to cut the isothermal lines, the temperature of the conductor will vary with the current for different diameters. Thus, a curve representing 4,000 ampères to the square inch, while good enough for small wires, rises rapidly from isothermal to isothermal. The Board of Trade rule is one of this class. It allows 2,000 ampères to the square inch up to 10 ampères, and above that, 1,000 to the inch. It is evident that similar drops must be made at intervals for heavier currents, or the curve will eventually reach the isothermal of fusing temperature.

Rules of this class are based on the question of resistance, and it must be assumed that the conductor is surrounded by a perfect conductor of heat, which would probably be an equally good conductor of electricity. Sir W. Thomson's rule gives ·5 ampère per square millimètre.

The graphical tables (Figs. A and B) are really projections of

a solid figure somewhat like a ploughshare; the three dimensions of length, breadth, and thickness being the scales of current, diameters, and temperatures. The isothermal lines show the shape of this surface, as the lines of an end elevation of a ship show its shape. It appears very possible that an actual model of this surface may yield some interesting results like the thermodynamic model of Professor Gibbs.*

If a curve be plotted (Fig. C) from Professor Forbes's Table I., taking temperatures as abscissæ, and the currents which will raise a millimètre wire to these temperatures as ordinates, the intermediate temperatures may be read off, and the curve may be extended to 100° C. without introducing new error. It is thus that the points on the millimètre line of Fig. A were found. But crossing this line also are the curves of $C = d^{\frac{3}{2}} \times a$, where a is some arbitrary constant. Hence may be drawn a curve (Fig. D) with these constants as ordinates, and the temperatures as abscissæ. Hence also the constant to use for any required rise of temperature may be found at a glance.

But if this curve be extended as far as 525° C., viz., the dull red-heat point determined by Mr. Preece, his constant, 14,590, lies considerably off the line, showing that the formula of Professor Forbes, or rather the value of the emissivity, does not hold for this high temperature, which confirms his deduction from his calculation on the fusing point of a lead wire.

One or two experiments between 100° and 500° C. would give the data for plotting the whole of this curve, and from this the emissivity could be determined for any temperature under red heat.

The relation between the temperature and the constant is approximately satisfied by the empirical formula

$$C = d^{\frac{3}{2}} \times 54 \times \sqrt[22]{t^{\circ}},$$

where C is the current in ampères, d the diameter of the wire in inches, and t° the temperature above the surrounding air (at 20° C.).

* Maxwell's "Theory of Heat," p. 195.

The departure from the law with very small wires is shown by Mr. Preece's experiments, where a wire of 8 mils took a current of 3·85 ampères to raise it to a dull red heat, instead of 3·35, as the curve would indicate.

The above remarks apply only to the case of a bare, black copper wire surrounded by air, which, being a non-conductor of heat, allows the heat to escape only by radiation.

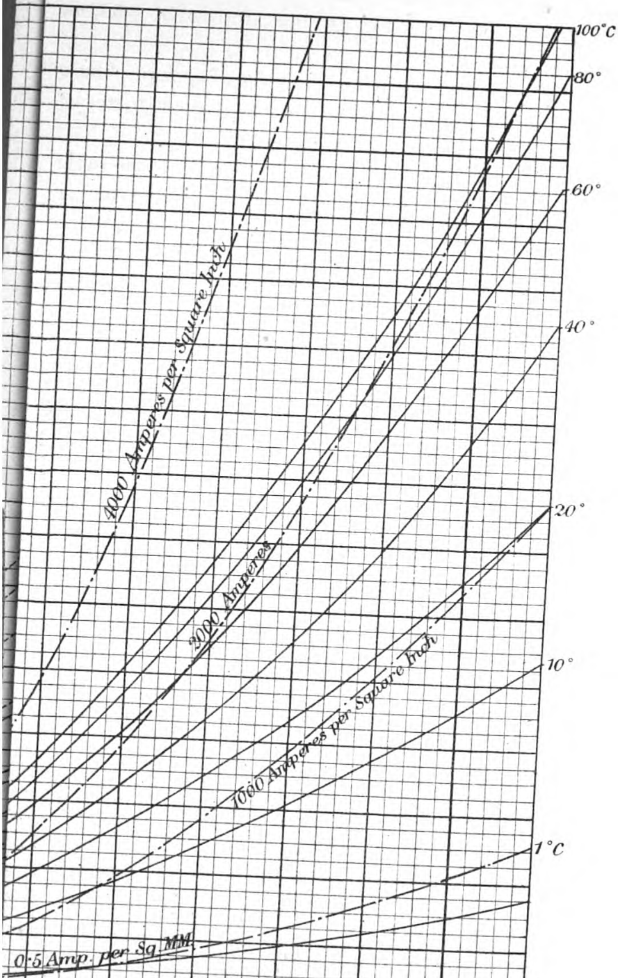
On applying precisely similar graphical analysis to the table for *subaqueous and aerial cables* (insulated), it appears that the formula may be simply expressed by

$$C = d \times \cdot 82 \times \sqrt[22]{t^\circ},$$

where d is the diameter in centimètres, and t° the excess of the temperature of the cable above the air.

An extremely simple rule may be deduced if 32° C. be taken as a safe rise of temperature. *The diameter in inches, multiplied by* 1,000, *gives the current in ampères.* It appears that 3,000 ampères is the point at which the laws of safe current and economical current coincide. This rule is therefore not economical for conductors of less than three inches diameter.

Mr. HERBERT TAYLOR: Some results of mine, published by Messrs. Clark, Forde, and Taylor, have been referred to. They were made on three gutta-percha-covered wires buried in sand and sawdust. Currents of known strength were sent through those wires, and the increase of resistance and temperature observed. It appeared that the energy required to produce a given rise of temperature varied as the surface. The observations were continued until the gutta percha showed signs of softening. Since then I have been working many months on the subject of bare wires, and I hoped to publish the results before now, but the arithmetical work has been very heavy, and I have not yet got out all my reductions. Professor Forbes has pointed out the relation existing between the diameter of a wire and the temperature produced in it by a given amount of energy. I have found that for a given temperature the energy per unit length is not directly proportional to the surface or diameter, but to the *square root* of the diameter, and in all the wires I have dealt with I have not found a variation of three per cent. from this law. Of



100°C
80°
60°
40°
20°
10°
1°C
4000 Amperes per Square Inch
2000 Amperes
1000 Amperes per Square Inch
0·5 Amp. per Sq. MM.
0·4
0·5
0·6
0·7
0·8
0·9
1 Inch Diam.

course I have not had very large wires to work with,—120 mils is the largest, down to 30 mils,—but in every case observed that that law subsists within three per cent. I cannot imagine any better way of working on such a subject than by means of the temperature coefficient of the wire, and the measurement of the increase of resistance produced by the current. It is perfectly easy to measure the resistance with sufficient accuracy, and then determine the coefficient of a particular wire, and so make absolutely sure of your rise of temperature to one per cent. or less. You can measure the resistance to one in ten thousand very easily. My method was to stretch the wire to be tested along a room by silk bands, to form one arm of a Wheatstone bridge, the adjacent arm being a German silver wire of very large surface immersed in a vessel of paraffin oil of high insulating property, and which was constantly stirred up by a little dynamo with a screw propeller at the end of the spindle. There was not a difference of one-tenth of a degree C. in any part of the vessel, and thus it was quite certain that the temperature of the German silver, and therefore its actual resistance, was accurately known.

The *opposite* sides of the bridge consisted of a "slide" resistance of 100,000 ohms; so there could be no doubt that the amount of current passing through these sides was negligible in comparison with that through the standard and wire, the resistance of which did not exceed one-tenth or one-twelfth of an ohm, and which were of course in parallel circuit with the "slides."

Referring to Professor Forbes's Table I., my own results differ very considerably from those quoted by him as derived from Mr. McFarlane's coefficient. I only take a single example: for a bright wire ·285 centimètres in diameter, the current required by calculation to raise this wire 9° C. is, according to the table, 14·2 ampères, whereas I find in actual experiment that 20·1 ampères are required. To raise the same wire 25° C. would require, by calculation on the basis of the table, 23 ampères, whereas I find the actual current required is 36 ampères. I have also done some work on gutta-percha-covered wires, but the results of my experiments are not yet in a fit state for publication.

Mr. R. E. Crompton: I hesitate to contribute anything towards this discussion, because I, unfortunately, cannot do justice to the mathematical side of the paper, as I have not had that time which I think it requires. I can only speak on what Mr. Preece has termed the "practics" of the question: in that I can tell you the results of passing large currents through large cables for the longest period of time that we are able to pass them under existing circumstances.

I should first point out that we are under deep obligations to Professor Forbes for introducing this subject—a subject which will be an interesting one to us for many years to come. I hope there will be many papers on it, for the subject is very new. We are told wherever we go that we are in the infancy of the electric light, and although I always challenge that statement, thinking that we know a good deal about it, I must admit that we are certainly in the infancy of this section of it; for the interesting part of Professor Forbes's paper really relates to large conductors, and these large conductors, I may say, are almost unused up to the present time. Mr. Edison, in America, is the only man who has used them to any considerable extent. In the absence of his testimony we cannot compare any of the figures arrived at by Professor Forbes with results obtained from actual practice. I think that Professor Forbes has made his paper less interesting to us than it would have otherwise been, because he has gone too far. He has argued the question out on a conductor of dimensions far exceeding anything we can conceive necessary for electric-lighting purposes for many years to come. Mr. Preece has criticised him so severely on this point that I think it unnecessary to further notice it, because I think that it is extremely improbable that the cables on the two sides of the street would collectively have to carry more than 5,000 ampères. I have had to lay out a good many districts in London, in Birmingham, and in other towns, for the purpose of lighting various areas by electricity, and I find that in all cases the factor which governs the size is the height or number of floors in the houses, and consequently the number of lights that can be crammed into a given ground area. For instance, such a district

as the neighbouring one of Victoria Street, which is principally laid out in flats, will require very much larger cables to be laid in that street than would be necessary in a district consisting entirely of two-storied houses, possibly with gardens intervening amongst them. But I have always calculated that even the case of such a street as Victoria Street, with its houses of such great capacity for taking electricity, can be well served up to the radius that we have taken as the practical limit, *i.e.*, half a mile from the generating station, by conductors collectively of not more than five square inches of sectional area; and as I had obtained the following results from actual practice, I have always considered myself justified in allowing a current of 1,000 ampères per square inch to pass through the conductors for the short time during which the maximum current would be reached.

The only case in which we have had extended experience with buried conductors through which a heavy current passes for many hours consecutively has been at the Royal Courts of Justice.

We have there four cables laid in one trough. The cable consists of a strand of 19 wires, each wire being ·160 inch in diameter; the united cross sectional area of 19 wires is as far as possible ·406 square inch. The current we pass through each cable being 400 ampères, to all intents we pass 1,000 ampères per square inch.

These conductors have been working for a year, and during many foggy days the full current has been passed for $9\frac{1}{2}$ hours, and yet, as far as we have been able to ascertain, the cores have never been perceptibly warm at the time of ceasing work.

I think $9\frac{1}{2}$ hours is practically the limit of the time during which we may expect that the maximum currents will pass through the conductors when we light large towns. I therefore feel safe in believing that with conductors having large radiating surface, such as I should obtain if I employed conductors of flat section, 1,000 ampères per square inch can be occasionally allowed. I have always had before me the great advantages we gain by using flat conductors. In addition to those urged by Professor Forbes, I may add another, and that is their extreme cheapness, both as regards manufacture and cost of laying, when compared with any

form of stranded or rod conductor. The experiments necessary to obtain trustworthy data to check Professor Forbes's calculation, applied to the larger conductors, are so expensive and difficult to carry out, that we probably shall have to wait some time for them.

I do not quite follow Professor Forbes when he says that we can easily measure the rising temperature in the main by measuring its increased resistance. He perhaps does not realise that when the total resistance of a large conductor only amounts to several 1,000ths of an ohm, the measuring instruments must be very special in their character, and be particularly careful not to introduce errors due to bad contacts and the like.

I do not think that Professor Forbes's simile, when he compares the large electric mains we must be prepared to use, to the huge gas-pipes now laid down for conveying gas from the suburbs to the centre of London, is a good one in any sense. He perhaps forgets that these large pipes are *reservoirs* of gas; as well as being conductors, they act as an extension of the gasometer. Again, the manufacture of gas is a noisome operation, and cannot be carried on at or near the centres of towns, whereas the manufacture of electricity, we hope, will not be a noisome operation, and consequently there will not be the same necessity for conveying it from considerable distances.

The only other point on which I can contribute anything of interest towards this discussion is in the case of the bare, or nearly bare, copper conductors that we use in the armatures of dynamo machines. There the extent of the heating of the conductors is to manufacturers of dynamo machines a matter of paramount importance. I have made many observations of the rise of temperature in the armature coils of my own machines, as well as with those of other makers, with this result, that for small wires of ·057 or ·06 of an inch in diameter, it is safe to pass currents of 4,000 ampères per square inch, the cotton or other insulating material and everything remaining in perfectly good order.

Professor FORBES: In the armature?

Mr. CROMPTON: Yes. Again, through ·160 inch wire, commonly known as No. 8 gauge, we have passed for long periods a current

up to 3,000 ampères per square inch or thereabouts, and I believe those figures correspond with those obtained by other makers. It has been very interesting to us to notice the results arrived at by Professor Forbes, Mr. Bottomley, and others, which pointed out that it is better to insulate the conductor up to a certain thickness than to leave it bare: thus, by increasing the radiating surface, we are able actually to keep the wire cooler than when bare. There is no doubt that this is a most important fact, and well worthy of our careful attention. But I should point out that insulating the wire with cotton, and not saturating that cotton with any kind of varnish or material which will fill up the intercellular space between the fibres of the cotton, will certainly not give this result, or radiate so well as a wire covered with a continuous and homogeneous non-conductor of electricity, but which at the same time is a fairly good conductor of heat.

The PRESIDENT reminded the members present that there were two papers upon which discussion was desired, but that so far the remarks had been directed to one only.

Mr. CARUS-WILSON: I had been looking forward to Professor Forbes's paper for values of the coefficient of emissivity, as that is the only variable in this problem for which we have not any satisfactory formula. I was rather disappointed at not finding any definite statement as to the value of this function of t. Suggested for the value of the coefficient (θ), the equation, $\theta = \alpha + \beta\,\mathrm{T} + \frac{\delta}{\mathrm{T}}$, a hyperbola, as being more like the actual value than McFarlane's parabola, $\theta = \alpha + \beta\,\mathrm{T} - \delta\,\mathrm{T}^2$.

Mr. WYLES suggested that since it was convenient to use flat bar copper for electric light mains [Mr. Crompton's speech], it was at once possible to compile a table in which the safe working current would be almost directly proportional to the weight per unit length of copper used for a standard thickness, because the surface for radiation and convection increases in nearly direct proportion as the weight, the thickness of course being constant.

Mr. T. H. BLAKESLEY: May I speak to one point in Professor Forbes's paper. In investigating the laws connecting the current with the rise of temperature, or with a difference

of temperature between the outside surface and the air, I would point out that Professor Forbes neglected the question of the increase in the internal temperature, the temperature gradient as points nearer and nearer the centre are considered. When any body is emitting heat there is a temperature gradient inside, and the inside of any conductor giving out heat must be necessarily much hotter than the surface. This is very strikingly exhibited in the case of lead fuses. Very often in making experiments with lead fuses you will find that the fuse bursts in a curious way, and this is due to the interior of the lead conductor being at a much higher temperature than the outside. Lead is one of those metals which expand in melting, so that the inside exerts a bursting force on the outside, and that is why a lead fuse often explodes as it does. I have followed out Professor Forbes's reasoning on these bare conductors, taking this point into consideration, and I have arrived at a formula which I have applied to Professor Forbes's own experiments as he has given them, and it answers very well.

The formula is

$$C^2 = \cdot 162\, d^3\, t - \cdot 00165\, d^4\, t^2.$$

t = difference in temperature between the surface and the external air in degrees centigrade.

d = diameter in millimètres.

I find that expresses very closely the actual facts, and if the second term on the right hand is omitted the law takes the form Professor Forbes finds his theory would indicate. The second term is the first approximation to a correction due to temperature gradient within the conductor. You must not press it too far, for that which I have put down as a constant really involves the radius and difference of temperatures in a certain way which, when these quantities are small, does not appreciably alter the value; but the thing must not be pressed too far.

The PRESIDENT: We have had some very interesting points brought forward in this discussion, to which I wish to add a few remarks before calling on Professor Forbes for his reply. Mr. Preece said that we are unfortunately condemned to use "mils." It seems to me that if we make up our minds to do away with

the use of mils we can easily do so, seeing that ·04 inch make a millimètre. We can take a mil as ·025 millimètres, *i.e.*, 40 mils make a millimètre. The wire that Mr. Crompton told us he uses at the Law Courts is ·160 of an inch, which comes out as exactly 4 millimètres, so that in fact it would be very much more convenient to use millimètres than to use mils for the wires that we are constantly employing.

We may perhaps look upon the loss of heat from coated wires in this way: the coating enlarges the wire; it is not a good conductor of heat any more than it is a good conductor of electricity, since these two properties are usually possessed by the same substance; but at the same time it is a better conductor of either electricity or heat than the same thickness of air. Therefore we are making a larger and a better conductor than previously occupied the space surrounding the wire, and we may expect that more heat will be conducted away from the wire and radiated from the outside surface, especially as that surface is probably a much better radiator than the bright polished surface of the wire itself.

Mr. Bottomley has pointed out in the *Electrician* last week that a wire will be kept cooler by a covering if its radius be less than $\frac{k}{e}$, where k is the thermal conductivity and e the emissivity of the substance of the coating. He also arrives at the conclusion, from his experiments, that e is much larger with small wires than with large ones, and finds that $C^2 = \mu^2 e\, d^3$, where d is the diameter of the wire, and μ^2 varies directly as the difference of temperature, and inversely as the specific resistance of the wire.

I will now ask Professor Forbes to reply.

Professor G. C. Forbes: Before replying to the remarks which have been made this evening on the paper I brought before you, I must express my sincere appreciation of the very kind reception to the paper when it was read. That reception was most utterly unexpected by myself, because I thought I was dealing with a matter which was perhaps too theoretical to be of interest to a great many of the members. I knew that the method I adopted was the only way to treat the matter, and I believed that the subject would be advanced to some extent by reading the paper,

and it was extreme gratification to me to see how kindly the paper was received when it was read.

In my paper I tried to give as complete a historical summary as possible of what had been done before in the matter. Since that paper was in print other work has been done, and other facts have come to my knowledge, and therefore it is only right that I should draw attention to these things which were not included in my previous historical summary.

The first point has already been alluded to, viz., that the theoretical law which I gave in 1882, of the current square varying as the diameter cube, had already been mentioned in the *Electrical Review* of June of the same year, in an article on incandescent lighting, by Mr. Kempe. Secondly, I ought also to speak of a letter which has appeared in the *Electrician* for 19th April, 1884, from the pen of Mr. Bottomley. I know Mr. Bottomley far too well to imagine for one moment that he would not give credit where it happened to be due; and therefore, if he does not seem to give me due credit, I am sure it is done in pure ignorance of what I had shown in my paper communicated to you last month. But at the same time it appears that he has been working on the subject of insulated wires for some time in an experimental way, and arrives at exactly the same result as I announced to you last meeting, which was that the insulated coating assists the cooling of the wire up to a certain diameter; and it is extremely interesting to find this confirmed. He also confirms the results I obtained in 1882, that with thin wires a larger current can be carried without overheating than what the theory, as there given, admits of.

Mr. Bottomley has referred to the theorem which Sir William Thomson has marked out, showing what I said about insulated conductors keeping cooler than bare conductors up to a certain temperature; and Professor Adams has just now repeated it, when he said that the conductivity ought to equal the radius in order to arrive at a critical diameter. Now my own work differs only by giving a fuller statement; for the statement made is true only when the diameter of conductor is fixed, and the radius determined by the above statement is that of the insulated cable.

Again, there has come to my knowledge within the past

fortnight a table which was calculated for the *Electrician* by Mr. Geipel, to which reference has already been made, and which was shown in the instructive table that Mr. Trotter explained to us. I am very glad indeed to see that this journal, which published an erroneous table giving the *safe* working current as being proportional to the sectional area of the conductor, has seen well to bring out so much more useful a table as this one of Mr. Geipel's, which is one that seems likely to work fairly well in practice, and which is founded upon a rational basis. I ought also to have mentioned that the subject was touched upon by Sir W. Siemens and Sir W. Thomson in 1876-77.

Taking the gentlemen who have spoken in order,

First, Mr. Preece told us, when he began his criticism, that he was going to abuse my "practics"—that safety is one thing and that economy is quite another thing; and that is exactly what I agreed with him in. When I was working out the paper, I had nothing whatever to do with economy: we constantly meet with the question of safety, and in the paper I was only concerned with that question. As to Mr. Edison having got only three lamps per horse-power in his installation, that is exactly what I maintain, viz., that without applying theory you will end in failure; and that is exactly why, finding there were no practical rules for it, I have been trying to improve the "practics" by introducing formulæ which are a far nearer approach to the truth than anything which we have had hitherto; and when we are almost totally devoid of practical results all we can do to start the "practics" of the subject is to use our theory to the best of our powers.

Then Mr. Preece has made many remarks to show how utterly absurd my "practics" are, and he gave figures about the size and cost of the conductor, its insulation, and so on. Well, I chose a wide conductor such as that because, to carry out the Provisional Orders granted by the Board of Trade, it is necessary to cope with such questions; and it was when I saw people dealing with Provisional Orders who knew nothing about the size of wires for the required current that I felt really bound to take up the question, and it was for their guidance that I worked out these

figures; and if in this I have reached the *reductio ad absurdum* which Mr. Preece has shown in such a telling way, I think I have at least done some good by showing the enormous size of cables which must be used for such purposes, and which I do not think have been taken into account by those people who have sought Provisional Orders.

As to the cost of the conductors, Mr. Preece says that it would be £700,000 per mile for the two copper conductors, not using the earth as a return, and this I believe is about correct. But if iron of the same conductivity were used, it would be about one-fifth part of the cost of copper, or £140,000 per mile. Now that is for 100,000 lamps, and therefore it means £1 5s. for each lamp of the installation, and the interest on that comes to about 1s. per year per lamp, and I do not think that at all excessive; but of course copper would not be obtainable in the market if we were going to lay down such conductors.

Mr. Crompton: You have left out the cost of distribution altogether.

Professor Forbes: I do not think 1s. enormous when you come to compare the cost of the lamp.

Mr. W. H. Preece: But that is six times the resistance at one-sixth the cost.

Professor Forbes: No; it is about one-twentieth the cost at the present rate—rather less. Mr. Preece arrives at the final conclusion that it will not be possible to light districts in this way, but that it would be necessary to use high tension currents *after something has been invented* to enable us to use high tension currents. Well, his "practics" may be much better than mine; but I have been dealing with things as we have them, and not with things that may happen in the future.

I think Mr. Preece made a slip of the tongue when he said that 65,000 horse-power was wasted. I think he meant 6,500, but it is not so much as this.

Mr. Taylor has arrived at the result that C varies as $d^{\frac{5}{4}}$, and this is a confirmation of my experimental result that the diameter must be raised to a power between 1 and $\frac{3}{2}$. These results are useful in telling us what the law is for wires of small diameter,

which are subject to convection currents, and where the cooling effect is not proportional to the surface. But when we come to wires of large diameter, then the cooling effect will be proportional to the surface, and the theoretical law which was published in the *Electrical Review* also becomes the correct law for wires of large diameter, in accordance with Mr. McFarlane's and Mr. Nichol's experiments.

The same thing explains why it was that Mr. Taylor found for wires of small diameter that the actual current which was carried was larger than that which I have given in my tables. I have stated in my paper distinctly, that with wires of small diameter a larger current can be carried than that which is given in the tables, owing to the fact that the convection is greater for the small ones, but I have attempted no accurate formula for these small wires, as Mr. Taylor has done.

Passing to the remarks of Mr. Crompton, I agree with a great deal of what he has said about the comparison with gas mains, and I confess that my own views have been modified since I read my paper; although I still maintain that the analogy of the advances which have been made with the advances with gas will help us very much in the future applications of electricity, if we pay attention to them.

I am glad to at last get particulars of the Law Courts cable written down in figures; and I have no doubt that the large current which it has been carrying is partly due to the very large amount of insulation it has, which requires all to be heated up, and which gives a large surface to carry off the heat. That probably is the true reason why it has not got heated much in the 9½ hour periods during which it has been working. It will be a matter of the greatest interest if Mr. Crompton does carry out the experiment he proposes of running for at least 24 hours. We should all wish to hear the results of such an experiment; and I am perfectly confident that we should know more about the subject after such a lengthened trial, especially on a cable which has so many distinctive features, some parts being close to the surface of the ground, and some at depths varying from 6 to 8 feet below. Of course it would take days and days, I might almost

say months, before the cable would arrive at the maximum temperature—certainly many days.

Then Mr. Crompton spoke about wires on armatures consisting of nearly bare copper wire, and said he found it safe to pass 4,000 ampères per square inch in one size of wire, while in another size of wire he passed 3,000 ampères per square inch. These are very interesting facts, and it would be also very interesting if he would work out from them what is the emissivity of an armature coil: it is a very different quantity when the armature is in motion to what it is when at rest. Of course the cooling action of the air makes the emissivity much greater when the armature is rotating, and this leads me to the last section but one of my paper, in which I dealt with coils. The difference between Mr. Crompton's result and that obtained by my formulæ arises from the fact, that instead of using my multiplier ·25, we shall have to increase it to a certain quantity depending upon the velocity of rotation of the armature, which it will be my object, and I hope that of other people, to determine in future. Then Mr. Crompton says, that insulation does favour cooling he has found by actual experiment.

Then Mr. Carus-Wilson, I am afraid, was mixed up in the diagram he gave us. He has been trying to get a formula which exactly fitted into every one of the experiments, and I think that is why he obtained a curve which changes its curvature so frequently.

My last remark is about Mr. Blakesley's question as to whether the apparent variation from theory for thin wires does not depend upon the change of temperature as you go inwards, the wire being much hotter in the interior than outside. This is a very complicated question; and when I first met with that difficulty Professor Everitt worked it out mathematically, and found that it had nothing to do with the result. I anticipated the question, and, having mislaid Professor Everitt's notes, I worked out the problem, and brought the solution with me in case it was raised, and I find that it does not affect the problem in the least.

Mr. Blakesley: I thank you very much for the attention given to my paper, and I am very glad that, after it has been for a month before the Society, no one has found anything to impugn in it.

A hearty vote of thanks was accorded to Professor Forbes and Mr. Blakesley for their respective papers.

A ballot then took place, at which the following were elected:—

Members:

J. Farquharson. | D. W. Lane.

Associates:

Charles M. Dorman.
Allan Westly Rose.
William Geipel.
Arthur Hullah.
J. A. Chambers.
Edward William Brown.
Charles John Phillips.

Foreign Member:

A. Master.

The meeting then adjourned until May 8th, when the President announced that a paper would be read on "A Method of Eliminating the Effects of Polarisation and Earth Currents from Fault Tests," by Henry C. Mance, with some supplementary remarks and illustrative experiments by Mr. Latimer Clark.

REMARKS ON PROFESSOR FORBES'S PAPER.

Forwarded by ANDREW JAMIESON, C.E., F.R.S.E.

The formulæ given by Professor Forbes are no doubt very interesting and instructive to consulting electricians, but I think of less use to practical men engaged daily in wiring houses or ships than rules like the following which I give, viz.:—"The conductor shall not be less than ·001 square inch per ampère carried, and the insulation resistance of each circuit not lower than 1,000 ohms per volt generated by the dynamo," rather than in accordance with any particular mathematical formula. Along with the rule I enter in the specification, or send to the contractor, a copy of the following table, wherein only the most common and handy sizes of leading wires are used :—

TABLE FOR SIZES OF SHIP LEADS.

WELL INSULATED.

For Swan 20 candle-power or Edison 16 candle-power Lamps.

Maximum number of lamps to be carried.	For 45 to 60 volt lamps, requiring 1·5 to 1·1 ampères,	For 90 to 110 volt lamps, requiring 0·8 to 0·6 ampère.
	Board of Trade Wire Gauge Branch Leads.	Board of Trade Wire Gauge Branch Leads.
1	No. 18 wire	No. 20 wire.
2	„ 16 „	„ 18 „
3	„ 15 „	„ 16 „
10	„ 10 „	„ 12 „
	Main Leads Strand.	Main Leads Strand.
20	7 No. 15s	7 No. 16s
25	9 „ 15s	7 „ 16s
45	19 „ 16s	7 „ 14s
60 to 70	19 „ 14s	19 „ 16s
	and so on.	

It is seldom that on board ship more than 300 ampères are generated by any single dynamo; and, as several main leads usually spring from the dynamo room to various parts of the ship, the currents in any one of these seldom exceed 100 ampères, so

the wires do not vary greatly in size, and generally four sizes will wire a ship.

The rule with the table has been calculated for the best class of insulated leading wire—conductor covered with (1st) felt, (2nd) india-rubber, (3rd) tape, (4th) waterproof tape—according to the rule that the interest at 5 per cent. plus depreciation at 10 per cent. plus insurance at 7 per cent. on the prime cost equals the price paid for energy lost per annum in heating the wire; the actual cost of coal, etc., for a ship trading to India, and the average time the lights are used, etc., being taken into account. The rule, it will be observed, is a perfectly safe one; and only from 2 to 4 per cent. of the total energy developed can be lost in heating the wires in even the largest first-class passenger steamers.

The One Hundred and Thirty-fifth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 8th May, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and approved, the names of new candidates were announced and suspended, and donations to the Library from H. Bramall and the Physical Society announced.

The following paper was then read by Mr. Latimer Clark:—

ON A METHOD OF ELIMINATING THE EFFECTS OF POLARISATION AND EARTH CURRENTS FROM FAULT TESTS.

By HENRY C. MANCE, C.I.E., Member.

In introducing the paper, Mr. LATIMER CLARK said: I have to express the great pleasure I feel in being requested by Mr. Mance to bring his paper before you, as it is one of considerable practical utility, and full of interest to those engaged in the repairs of submarine cables. Before I commence the paper, I will just briefly point out on the diagram the nature of the test. Mr. Mance uses the ordinary form of Wheatstone bridge, and measures the resistances of his line in the ordinary way, twice over with two different proportional resistances, such as 100 : 100 and 10 : 10, or 1,000 : 1,000 and 100 : 100, making any variation in the external resistances necessary to obtain a balance by means of slide resistance coils.

We have on the table, through the kindness of Messrs. Latimer Clark, Muirhead, & Co., some instruments which have been specially constructed according to Mr. Mance's design, for the purpose of making this kind of test, and they will presently be employed in making a series of tests in illustration of the paper.

Mr. Mance's paper is as follows:—

Considering the large number of formulæ already placed at our disposal in the text-books, it may at first sight appear rather presumptuous offering for your consideration another method having

for its object the easier and more accurate localisation of a fault in a telegraph cable when loop or discharge tests are not available.

I may say, however, that the method is intended to supplement rather than supersede any of the existing formulæ commonly used to ascertain the resistance up to and through a fault. You will, I am sure, agree with me that navigating a vessel in mid-ocean would be rather uncertain work if we had to guess every day the necessary correction for the sun's declination; yet there is a striking analogy between this position and that of an electrician testing a broken cable subject to earth currents. Granted that all the recognised formulæ are theoretically and mathematically correct, you must first catch your hare—that is to say, get a true test before you can apply them. If the measurements from either side the fault are not equally affected by the fault resistance and electro-motive forces in the cable, there will be an error, sometimes a very considerable one. The question of fault resistance can be dealt with separately. The immediate object of the method I am about to describe is to eliminate whatever error may be due to polarisation and earth currents, and thus do away with two great sources of uncertainty.

Now some one may here exclaim, We already have a correction for an electro-motive force arising in the cable!—and so you have; but, with the formula to which you would refer, the electro-motive force in the cable is supposed to remain constant in strength and direction while both positive and negative readings are being taken. This is precisely what never occurs in the case of a cable making partial earth, as the polarisation current will change with every reversal of the testing battery, and the resistance of the fault itself may be seriously altered by the application of a positive current.

In the absence of any other formula to correct tests taken up to a complete break, I am convinced that those who, like myself, are frequently engaged in the repair of faults at sea will find this method particularly useful. Its simplicity and the rapidity with which it can be applied renders it most suitable for general use under circumstances when time is of value and prompt decision is absolutely necessary.

To test quietly on shore, where you may be able to bide your time and take advantage of any favourable opportunity when the cable happens to be free from earth currents, to try the different methods provided by the text-books, is comparatively a simple matter; but to cut in on a cable, say, between two faults,—to be testing with the knowledge that the cable is old and incapable of standing a strain, the ship possibly rolling and the cable staff waiting for orders,—is quite another thing. This is the trying time for an electrician. At such times the questions present themselves for immediate answer: Shall I splice up again? Pick up to the east or west? Is the cable broken close to the ship or half a mile distant? Must I buoy one side, or can I hold on while I wind in the intermediate bit? As a general rule, the electrician has only the tests from his own side to rely on, and under these circumstances any formula which will strip the tests of the increment due to polarisation current, leaving bare only the actual resistance in the circuit tested, is certain to be found of the greatest service.

Although applicable to tests taken through long lengths of cable, the value of the method becomes more clearly apparent when the distance does not exceed 1,000 or 1,500 ohms. The results given at the conclusion of this paper, to which I would invite your attention, are for the most part obtained through shorter lengths, where, in consequence of the greater strength of polarisation currents, the necessity for some correction is more urgently necessary. On the other hand, I see no reason why equally satisfactory results should not be obtained through very long cables, provided the earth currents are strong and steady.

The utility of the method can be tried with the ordinary bridge apparatus and an artificial fault; but when submerged cables are being dealt with, there is the risk of changing earth currents, and greater chance of variations in the fault itself. To reduce these dangers to a minimum, some modification in the arrangement of the proportion coils must be made, and the addition to the 10,000 box of a small sliding resistance of single ohms, to obtain rapid balance, will be found a great convenience in this or other systems of testing faults.

The method is based on the following principles:—

Two observations are required with the *same* current, and the observations should be taken as rapidly as possible after each other.

The testing battery (the internal resistance of which should be known) must not be disconnected *or reversed* during the two observations.

Between the first and second observation, R_1 and R_2, the resistance of at least two of the branches forming the bridge must be altered with the object of obtaining fresh balance.

The simplest plan, giving the best results and requiring the least trouble in calculating, is to use two pairs of equal proportional coils of different resistances for the two observations, rapidly substituting the one pair for the other between each test, and readjusting the 10,000 box, R, with as little delay as possible.

In practice I generally use the hundred and thousand pairs of proportion coils. I commence by observing the resistance with the smaller pair of coils, *continuing the test until the resistance of the fault appears fairly steady*, when, balance being obtained by adjusting the 10,000 box, R, I short-circuit the bridge galvanometer for an instant while switching from one pair of proportion coils to the other, and again produce balance by readjusting R as quickly as possible. This operation can be repeated as often as desired, and the most likely pair of readings selected for correction by the following formula :—

Let the reading obtained with the $\frac{100}{100}$ proportion coils $= R^1$
And the reading obtained with the $\frac{1000}{1000}$ proportion coils $= R_2$
Let the resistance of *one* of the smaller proportion coils ... ... $= 100 = P_1$
And the resistance of one of the larger proportion coils ... ... $= 1{,}000 = P_2$
Internal resistance of the testing battery ... ... $= r$
True resistance of cable tested up to and through the fault ... ... $= x$

Assuming that correct balance has been obtained during both readings, and that no appreciable change has taken place in the

electro-motive force in the cable, or the resistance of the fault between the two observations, we obtain from the above,

$$x = \frac{R_1 (2r + P_2) - R_2 (2r + P_1)}{R_2 + P_2 - P_1 - R_1}.$$

Example:—On grappling and heaving in the cable I got a steady test with a battery of 30 cells negative.

Using the $\frac{100}{100}$ coils, I obtained balance with 35 ohms.

" $\frac{1000}{1000}$ " " " 60 "

$$R_1 = 35.$$

$$R_2 = 60.$$

Internal resistance battery $r = 420$.

Then

$$x = \frac{35 \times 1{,}840 - 60 \times 940}{60 + 1{,}000 - 100 - 35} = 8{\cdot}6 \text{ ohms.}$$

We were evidently close to the fault I had come to repair, or a fresh break had occurred during the lifting of the cable. A fresh break under such circumstances would probably not have a resistance of more than 5 ohms; and a result of 8·6 ohms would imply that we had broken the cable at a point half a mile distant from the vessel. Being, however, in comparatively shallow water, this was improbable; I therefore assumed we were testing to the old fault, for which I allowed a somewhat higher resistance—say, 7 or 8 ohms. This proved to be the fact, as after winding in a very short length of cable one side of the original fault came on board.

In working this method, be sure that your battery is in good condition. High resistance in the battery is not desirable. Ascertain its internal resistance daily.

If the conductor is broken, the less the resistance between you and the fault, the more regular will be the results.

If the conductor is not broken, *and the fault is a small one*, sufficient resistance should be added at the nearest end to bring the fault near the centre. The tests from either side will then compare well with each other. In arranging this, the resistance of the batteries must not be overlooked, and it is therefore desirable that all stations should use similar batteries with approximately the same internal resistance.

When testing with the thousand to thousand proportion coils, the observations will generally, but not invariably, be higher than when using the hundred to hundred branches of the proportion box. This will depend on the earth currents existing at the time. The corrected result will, however, be approximately the same, although the readings may indicate an alteration of several hundreds of units in the resistance tested. The daily variations in the tests to a fault may of course be due to alterations in the fault itself, especially if it is a small one. The application of the correction will, however, at once show how much is due to the fault, and to what extent the tests are affected by other disturbing influences. Should the alterations be caused by the latter, there will be no material change in the corrected results.

Having provided a simple method by which the effects of the polarisation current can be eliminated, there is an excellent field for those with sufficient leisure to thoroughly investigate the question of fault resistance. The subject is one interesting to the experimentalist as well as to the practical electrician. Most of us have at various times made series of observations with artificial faults, for the purpose of ascertaining the correct amount of resistance to be deducted from fault tests, but the value of these observations would have been much greater had we been able to eliminate with certainty the effect of the polarisation current opposing the testing battery. For instance, an experimental fault tested on shore may offer an apparent resistance of, say, 50 ohms: of this, 40 is probably due to polarisation current. Could such a fault be tested immediately afterwards from a point 50 or 100 miles distant, it is by no means certain that its apparent resistance would remain the same: on the contrary, it will probably be something very different: the tests would then be affected by earth currents as well as those arising in the fault itself, and the resistance of the latter would apparently rise or fall with every change in the direction or strength of the natural currents.

I at first tried to ascertain the resistance of artificial faults by the method for testing batteries suggested by me in a paper read before the Royal Society in 1871, and described by Clerk Maxwell, Vol. I., page 411. In this method the current set up from the

fault itself is used instead of the testing battery. The results were, however, uncertain, leading me to suspect that with extremely weak currents either the resistance of the fault or the polarisation current was very variable. With weak currents the actual resistance in the defect itself appears to be higher than when the current is sufficiently powerful to cause brisk action at the surface of the fault. If the fault is a large one, the difference is not so marked. Take, for instance, as an example, a broken cable with not less than half an inch of copper exposed, the corrected results of tests taken with a continuous negative *or positive* current will come out about the same. You may be close to the fault or 500 ohms away from it, and the actual resistance offered by the fault itself will not vary more than two or three ohms. Results obtained with different battery powers will agree very closely, and you will be quite safe in allowing a resistance of from 7 to 12 ohms for the fault. A fresh break near the cable ship would probably offer a resistance of 5 or 6 ohms only, but for the same fault 1,000 ohms distant it would be advisable to allow at least 9 or 10 ohms.

In discussing the resistance of faults, I should wish it understood that my observations are based on results obtained in connection with the Persian Gulf cable, in which the conductor is a segmental copper wire 110 mils in diameter and weighing 225 lbs. to the nautical mile. The outside diameter of the percha core is 380 mils, the weight of percha per knot being 275 lbs. The sheathing consists of 12 No. 7 iron wires, and the complete cable weighs $3\frac{3}{4}$ tons per knot. The resistance of the conductor is about 6 ohms per mile.

When a cable is violently broken, or gradually gives way wire by wire under severe strain, it usually happens that both the percha and copper wire is considerably stretched at the fracture; the former contracts again to a certain extent, and the ends of the conductor are left exposed to the water on both sides of the break.

It frequently occurs that, after the sheathing has corroded, the conductor breaks under the strain without rupturing the percha, which, although considerably elongated, still preserves the insula-

tion. This kind of fault can, however, easily be localised by discharge tests, and need not be considered here.

I have no case on record, during an experience of several years, in which, after a *complete* fracture, the copper conductor, either on one side or the other, was not left exposed; we can therefore rely with a considerable amount of certainty on always having one end of a broken cable making sufficiently good earth to give satisfactory results. I usually find three-quarters of an inch of copper projecting beyond the percha at a total break, and this is more than sufficient to give a steady test. At the same time it is desirable that a test should be taken as soon as possible after the occurrence of a break, as, if the surface of copper exposed is very small, the first tests will be the most accurate, and the subsequent rise in resistance will suggest the advisability of allowing a few ohms additional on account of the fault resistance.

When the fault is very small,—say, having a resistance of two or three hundred ohms,—the application of a strong negative current will often send it up enormously in resistance. This is probably due to the bubbles of gas being unable to disengage themselves from the fault. Under these circumstances a small battery power will frequently give better results, but provided there is free exit for the hydrogen evolved at the fault, it seems desirable to test with a moderately strong battery. In localising small faults, it is most desirable that the testing stations on either side should be equally distant, as an addition of a few hundred ohms to the circuit will materially alter the resistance offered by the fault itself.

When a cable is wrenched asunder at a point where the iron guards are corroded through, the copper conductor is sometimes found touching the iron wire; this makes, however, but little difference in the resistance of the end, which never gives what is called *dead earth*. The approximate resistance of a few artificial faults of various dimensions and under different conditions is shown in the accompanying statement, and I have added some extracts from my testing diary, which, while giving the actual resistance of several faults removed from the Persian Gulf cables, serve to illustrate the value of the method and formula which form the subject of this paper.

Table showing Approximate Resistance of a few Artificial Faults of various sizes, with different battery powers and at various distances. Only one pair of readings were taken in each case. Faults were immersed in sea water.

Size of fault.	Battery. No. of cells.	Battery. r.	Line resistance up to fault.	Observed with $\frac{100}{100}$ coils.	Observed with $\frac{1000}{1000}$ coils.	Resistance in fault.	Remarks.
$\frac{3}{4}$ inch copper exposed	10	10	Nil	13	69	5	Tests all steady down to fault $\frac{1}{16}$ inch.
	30	220	"	21	47	9	
	60	440	"	18	32	3	
	10	10	500 ohms	546	598	8	
	30	220	500 "	539	565	8	
	60	440	500 "	528	542	5	
	10	210	Nil	42	96	10	
	30	420	"	35	60	8	
	60	640	"	20	30	5	
	10	210	500 ohms	555	595	9	
	30	420	500 "	540	556	14	
	10	10	2,000 "	2,100	2,135	16	
$\frac{1}{2}$ inch exposed	60	550	250 "	294	317	6	
$\frac{1}{3}$ inch exposed	10	10	Nil	30	157	11	
	30	250	"	41	88	9	
	30	250	1,000 ohms	1,087	1,130	10	
$\frac{1}{4}$ inch exposed	60	550	250 "	288	307	7	
$\frac{1}{8}$ inch	60	550	250 "	292	312	13	
$\frac{1}{16}$ inch	60	550	250 "	305	325	22	
Copper end flush with percha	60	550	250 "	317	342	26	With positive current, rose to over 1,000.

Extracts from Testing Diary, showing the difference in results obtained by calculating the distance of the fault from the ordinary uncorrected tests, and the same tests when corrected by the method and formula.

Example 1:—Jask-Gwadur cable, 1,752 ohms in length; fault 488 ohms from Jask; insulation imperfect; same battery power used at each station; and all tests taken with the distant end of cable insulated.

Apparent position of fault, calculated from the steadiest tests from both sides, with 30 cells and thousand proportion coils,

471 ohms from Jask.

Apparent position when calculated from same test corrected by my method and formula,

484 ohms.

A 5-cell test, uncorrected, gave distance from Jask as

391 ohms,

but after correction,

471 ohms.

By inserting some resistance at Jask, so as to bring the fault approximately to the centre, a 30-cell test from each end gave distance as

476 ohms,

or, with the observations corrected,

489 ohms.

This fault, when tested with 5 cells, had a resistance in itself of about 100 ohms. On increasing the battery to 30 cells, the resistance of the fault was observed to fall to about 60 ohms. The corrected tests always indicated a distinct rise in the resistance of the fault itself when testing through the additional resistance inserted for the purpose of bringing fault to the centre of the line.

Example 2:—Length of section, 2,235 ohms; fault 180 ohms from one end. This defect was also a small one, having a resistance of about 70 ohms when tested with 30 cells, the highest testing battery power permitted on the Persian Gulf cables.

The uncorrected tests gave distance as

138 ohms.

The corrected tests, after bringing fault to the centre of the line,

172 ohms.

Example 3:—Here the fault was very similar to the last, but steadier. The distance from ship to shore was 817 ohms, and the position of the fault 318 ohms from the shore. The tests on one side were taken from the cable ship while engaged in the repair of another fault.

First test, uncorrected result, 338 from shore.
„ corrected „ 318 „

During the second test the actual observations from each side were as follows:—

Line = 817 ohms.

Ship, $r = 175$, $R_1 = 590$, $R_2 = 610$, corrected = 567.

Shore, $r = 345$, $R_1 = 443$, $R_2 = 483$, " = 387.

$$387 - \frac{567 + 387 - 817}{2} = 319 \text{ from shore.}$$

In neither of the foregoing cases was the conductor broken; the tests, therefore, from either side were comparable with each other. The following extracts refer to instances in which the cable has been completely ruptured, and the resistance of the fault has had to be assumed from the steadiness of the tests.

Example 4:—A total break, 463 ohms from Henjam.

In this case the circumstances were somewhat peculiar. A precisely similar fault had occurred in the same locality (466 ohms from Henjam) about a year before, the tests being very regular, and varying only a few ohms from day to day. On that occasion the mean of a month's tests was, with the $\frac{1000}{1000}$ proportion coils,

492 ohms.

A single pair of tests with different proportion coils gave a corrected result of

480 ohms.

As true distance of fault was 466 ohms, there must have been a resistance of 14 ohms in the fault when this test was taken.

On the present occasion, in consequence of the existence of other defects, tests from one side only could be obtained. The following readings were reported by Henjam during the week preceding the repairs, thirty cells being used in every case:—

r	R_1	R_2	Corrected.
260	525	559	483
260	526	565	478
260	521	559	474
260	524	560	480
260	526	566	477
205	521	560	478
200	517	553	478
	Means ...	560	478

There is an error of 2 ohms to be deducted from these corrected results in consequence of P_1 and P_2 being less than 100 and 1,000.

Corrected mean result, therefore, 476.

As the true distance to the fault on this occasion was found to be 463 ohms, the resistance in the defect itself was about 13 ohms.

Now compare the corrected and uncorrected results for the two faults, which were of a precisely similar character and size.

During the first fault the $\frac{1000}{1000}$ tests were 492 ohms, and the fault was 466 ohms distant; during the second fault the mean of the $\frac{1000}{1000}$ tests was 560 ohms, the defect being half a mile nearer, or 463 ohms.

The corrected results were—

For the first fault ... ... 480 ohms.
„ second „ ... ... 476 „

Guided by the experience of the previous year, the tests being equally free from oscillations, it would have been reasonable to assume that the second fault was at least 50 or 60 ohms more distant from the testing station. The corrected tests, however, showed clearly enough that, nothstanding the higher tests, the second fault was probably the nearer of the two, and instead of looking for the fault ten miles away, the repair was effected with the expenditure of one-third of a mile of cable only.

Example 5:—Two inches of copper were exposed on one side, the other being partially insulated: the tests were extremely regular:—

R_1	R_2	Corrected.	
395	433	358	Mean. 362
398	431	364	
394	424	364	
392	421	363	

In these tests $r = 200$, $P_1 = 95$, $P_2 = 950$.

True distance of the fault was 351 ohms; the resistance in the defect itself was therefore 11 ohms.

Example 6:—In the next case taken from my note-book, I find the ends were exposed on each side of the break, either end having a resistance of 10 or 12 ohms. I give the whole of the

tests reported from the more distant station, Jask, during the last three days previous to the removal of the fault:—

r	R_1	R_2	Corrected.
185	687	725	640
185	697	740	644
185	690	730	641
185	678	708	641
185	706	757	639

Example 7:—A total break nearly 2,000 ohms distant. The observations taken with the hundred proportion coils varied as much as 100 ohms; those obtained with the thousand coils varied to a still greater extent; but the difference between the highest and lowest of ten corrected results was, I find, 19 ohms only.

On rare occasions a faulty cable can be tested when it is in a neutral state so far as polarisation and earth currents are concerned. This is purely a matter of chance: you can never tell when it is likely to be neutral: you never know when the tests are unaffected by these disturbing influences unless you happen to be working with different proportion coils, as in this method. No correction will be required when the earth current is in a contrary direction and exactly equals the polarisation current, a circumstance which will be indicated by the hundred and thousand readings being exactly the same.

Example 8:—Shows that when the tests are moderately steady the occurrence of a second fault which might pass unsuspected is at once indicated by the change in the corrected results. The first fault was about 655 ohms distant:—

r	R_1	R_2	Corrected.
290	693	711	666
272	707	735	666
270	716	748	669
220	680	712	638 Second fault.
220	685	715	645
170	700	740	651

The fourth test showed that a second fault had occurred; and the steady rise in the resistance of the last fault indicated the probability that only a small amount of copper was exposed.

I lifted the cable at the first fault, and, as I expected, found another total break four or five miles distant.

Example 9:—Here are some figures taken from the record of a fault which occurred near the shore on the Jask-Gwadur section, which had at that time a resistance of 1,763 ohms; the ends on both sides of the break were exposed.

Gwadur reported, neg., 1,936; pos., 1,886. Mean, 1,911.

This test being useless, I asked for negative tests only with two proportions, and obtained—

r	R_1	R_2	Corrected.
115	1,803	1,819	1,766
115	1,872	1,923	1,754

Here, although the R_2 observations differed over 100 ohms, the actual difference when corrected was 12 ohms only.

On the other side, Jask reported—

	r	$\frac{10}{10}$	$\frac{100}{10}$	$\frac{1000}{10}$
10 cells,	6	42	51	130
30 cells,	20	39	43	77

The following corrected results can be obtained from these readings:—

36 38 37 35 36 36

The fault was four miles distant = 24 ohms.

The fault had therefore a resistance of 12 ohms.

Further extracts are, I think, unnecessary. In every case, without a single exception, I have found the corrected tests obtained by this method give more regular and accurate results than those taken in the ordinary manner.

The correction is applicable to tests taken when the distant end of the faulty cable is put to earth. It can also be used for correcting ordinary conductor tests taken on a perfect line, two negative readings being required instead of one negative and one positive. For instance, one of the Karachi harbour cables tested as follows:—

Neg., 51 ohms; pos., 27. Mean, 39.

Or by this method—

$r = 17$; neg., $\frac{100}{10}$, 42 ohms; $\frac{1000}{10}$, 56 ohms. Result, 39.

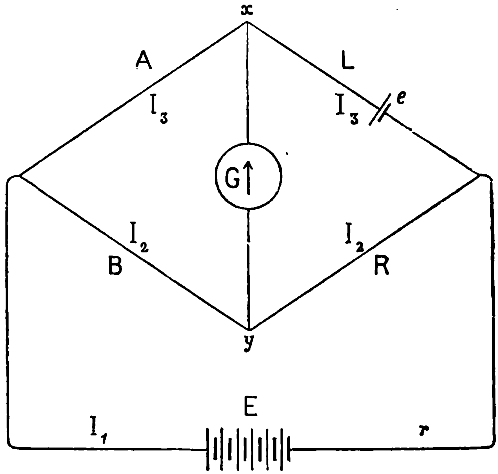
The Karachi-Gwadur cable tested—

Neg., 1,795; pos., 1,718. Mean, 1,756.

Or by this method—

$r = 17$; $R_1 = 1,782$; $R_2 = 1,794$. Result, 1,756.

MATHEMATICAL DEMONSTRATION OF MANCE'S METHOD FOR ELIMINATING FROM FAULT TESTS THE EFFECTS OF POLARISATION AND EARTH CURRENTS.



Given a bridge circuit as above, then by Kirchoff's law *the sum of the products of the intensities and resistances in all the wires forming a system of derived circuits is equal to the sum of all the electro-motive forces in the same circuit.*

The current in G during each reading is *nil* when balance is obtained; we may therefore discard it from our calculations, remembering that the potentials at x and y are equal.

Let e be the sum of the polarisation and earth currents;

E the testing battery;

r = internal resistance of testing battery;

A and B the proportion coils, the former being on the side of the unknown resistance;
L = line, the unknown resistance;
R = resistance unplugged to produce balance.

If I_1 = intensity of current in branch r,
I_2 = " " " B, R,
I_3 = " " " A, L,

Then

$$I_1 r + (B + R) I_2 = E \quad \ldots \quad \ldots \quad (1)$$

$$I_1 r + (A + L) I_3 = E - e \quad \ldots \quad \ldots \quad (2)$$

if e is opposing E.

$$I_1 r + (A + L) I_3 = E + e \quad \ldots \quad \ldots \quad (3)$$

if e is assisting E.

$$I_2 + I_3 = I_1 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

$$A\, I_3 = B\, I_2 \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (5)$$

$$R\, I_2 - L\, I_3 = e \quad \ldots \quad \ldots \quad \ldots \quad (6)$$

if e is opposing E.

$$L\, I_3 - R\, I_2 = e \quad \ldots \quad \ldots \quad \ldots \quad (7)$$

if e is assisting E.

$$I_2 = I_3 \frac{A}{B} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (8)$$

$$I_1 = I_3 \frac{A + B}{B} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (9)$$

Substituting in (1) and (6) the equivalent of I_1 and I_2 in terms of I_3, we have with (1) and (6)

$$\frac{E}{e} = \frac{\dfrac{I_3\, r\, (A + B)}{B} + (B + R)\, I_3 \dfrac{A}{B}}{R\, I_3 \dfrac{A}{B} - L\, I_3},$$

which becomes

$$\frac{E}{e} = \frac{(A + B)\, r + (B + R)\, A}{A\, R - B\, L} \quad \ldots \quad \ldots \quad (10)$$

or with equations (1) and (7)

$$\frac{E}{e} = \frac{\dfrac{I_3\, r\, (A + B)}{B} + \dfrac{(B + R)\, I_3\, A}{B}}{L\, I_3 - R\, I_3 \dfrac{A}{B}}$$

we obtain

$$\frac{E}{e} = \frac{(A + B)\,r + (B + R)\,A}{B\,L - A\,R} \quad \ldots \quad \ldots \quad (11)$$

This provides an easy means of ascertaining the strength of an earth current existing during a conductor test.

Take, for instance, equation (10),

$$\frac{E}{e} = \frac{(A + B)\,r + (B + R)\,A}{A\,R - B\,L},$$

$$e = \frac{E\,(A\,R - B\,L)}{(A + B)\,r + (B + R)\,A}, \quad \ldots \quad \ldots \quad (12)$$

but if the proportion coils are alike and each equals P,

$$e = \frac{E\,(R - L)}{2\,r + P + R} \quad \ldots \quad \ldots \quad \ldots \quad (13)$$

R being greater than L (the known resistance of the line tested), it is evident that e, the electro-motive force in the line, is opposing the testing current; but should e be assisting the testing current, the reading will be lower than the true resistance of the line, and with equation (11), using equal proportion coils, we obtain

$$e = \frac{E\,(L - R)}{2\,r + P + R} \quad \ldots \quad \ldots \quad \ldots \quad (14)$$

In consequence of the polarisation currents set up when testing to a fault, and the rapid variation which ensues when the testing current is reversed, it is better to measure the true resistance up to and through a fault by testing with a continuous negative current, and obtain two equations by rapidly altering the resistance of some of the branches of the bridge system between the readings. If this is done expeditiously, the change in the polarisation current from the fault is not appreciable, while the risk of the earth currents altering is no greater than in the ordinary positive and negative test.

Take the first reading, using the $\frac{100}{100}$ proportion coils (P_1), and call the resistance required to produce balance, R_1.

Then change quickly to the $\frac{1000}{1000}$ (P_2) proportion coils, and call the resistance unplugged to obtain balance, R_2.

As e and E are the same in each case, we start with equation (13), in which e is opposing E, and find that during the $\frac{100}{100}$ test

$$e = \frac{E\,(R_1 - L)}{2\,r + P + R_1} \quad \ldots \quad \ldots \quad (15)$$

during the $\frac{1000}{1000}$ observation,

$$e = \frac{E\,(R_2 - L)}{2\,r + P_2 + R_2} \quad \dots \quad \dots \quad (16)$$

Therefore

$$\frac{(R_1 - L)}{2\,r + P_1 + R_1} = \frac{(R_2 - L)}{2\,r + P_2 + R_2} \quad \dots \quad (17)$$

and

$$\frac{R}{2\,r + P_1 + R_1} = \frac{R_2}{2\,r + P_2 + R_2} + \frac{L}{2\,r + P_1 + R_1} - \frac{L}{r + P_2 + R_2},$$

from which we obtain equation (18),

$$\left(\frac{R_1}{2\,r+P_1+R_1} - \frac{R_2}{2\,r+P_2+R_2}\right) = \frac{L\,(P_2 + R_2 - P_1 - R_1)}{(2\,r+P_1+R_1)\;.\;(2\,r+P_2+R_2)},$$

or

$$L = \frac{\dfrac{R_1\,(2\,r + P_2 + R_2) - R_2\,(2\,r + P_1 + R_1)}{(2\,r + P_1 + R_1)\cdot(2\,r + P_2 + R_2)}}{\dfrac{P_2 + R_2 - P_1 - R_1}{(2\,r + P_1 + R_1)\cdot(2\,r + P_2 + R_2)}} \quad (19)$$

But suppose that instead of starting with equation (13) we commence with equation (14), in which the polarisation or earth current is acting in the same direction as the testing current, then with equal proportion coils we have during the $\frac{100}{100}$ test

$$e = \frac{E\,(L - R_1)}{2\,r + P_1 + R_1},$$

during the $\frac{1000}{1000}$ observation

$$e = \frac{E\,(L - R_2)}{2\,r + P_2 + R_2}$$

$$\frac{L - R_1}{2\,r + P_1 + R_1} \text{ being equal to } \frac{L - R_2}{2\,r + P_2 + R_2}$$

from this we obtain

$$\frac{L}{2\,r + P_1 + R_1} - \frac{R_1}{2\,r + P_1 + R_1} = \frac{L}{2\,r + P_2 + R_2} - \frac{R_2}{2\,r + P_2 + R_2}$$

$$\frac{R_1}{2\,r + P_1 + R_1} - \frac{R_2}{2\,r + P_2 + R_2} = \frac{L\,(P_2 + R_2 - P_1 - R_1)}{(2\,r + P_1 + R_1)\,.\,(2\,r + P_2 + R_2)}$$

which is the same as equation (18); from which we see that it is in this method immaterial whether the current from the fault is opposing or assisting the testing current, the same formula holding good whether R_1 or R_2 be the greater. As a general rule, the highest apparent resistance is observed when using the larger pair of proportion coils, P_2, but when testing a faulty

cable the reverse is sometimes the case in consequence of earth currents.

Returning to equation (19), we obtain from it

$$L = \frac{R_1 (2r + P_2 + R_2) - R_2 (2r + P_1 + R_1)}{P_2 + R_2 - P_1 - R_1}$$

$$L = \frac{2rR_1 + R_1 P_2 - 2rR_2 - P_1 R_2}{P_2 + R_2 - P_1 - R_1}$$

$$L = \frac{R_1 (2r + P_2) - R_2 (2r + P_1)}{P_2 + R_2 - P_1 - R_1} \quad \ldots \quad \ldots \quad (20)$$

This formula is the one commonly used.

Assuming the $\frac{100}{100}$ and $\frac{1000}{1000}$ coils are used,

$$P_1 = 100, P_2 = 1{,}000.$$

On the conclusion of the paper, Mr. Latimer Clark said: Mr. Mance points out that, although he prefers to use equal proportions such as 100 : 100 and 1,000 : 1,000, etc., there is no real necessity for their equality: any unequal proportions would do as well, but the formula in that case becomes more complicated, and the calculations less easy. He has been using the system for about four years in the Persian Gulf, but has not hitherto divulged the method even to his own staff, because he naturally desired to have a lengthened trial of it before making it public. Mr. Mance has therefore now had much experience with his system, and speaks of it with very great confidence. He remarks that the system has especial value in testing short lengths of cable in cases such as occur in ordinary cable repairs in shallow water. It is often very difficult to know when the fault is near you, and to avoid cutting your cable two or three miles short of it, or beyond it. One great advantage of the system is, that reverse currents are not used, and the two measures which form the test can be taken one after the other so rapidly as to be almost simultaneous. Celerity in taking such tests is of the highest importance, on account of the possible variation of earth currents; and with Mr. Mance's instrument the method is so rapid that there is no time for the earth currents to change; and since the current remains the same in direction, and only varies slightly in strength, there is no real reason why the polarisation should sensibly alter. Mr. Mance has designed a special apparatus or

form of bridge, of which there are several specimens on the table, and a diagram of the arrangement is also before you. [Mr. Latimer Clark explained the arrangement of the resistances and the passage of the current through the apparatus; and his assistant, Mr. McMullen, carried out an improvised test before the meeting. Assuming values, and working out a numerical example, Mr. Clark showed that the formula consists of three parts in the form $\frac{A - B}{C}$, and that much of the calculation may be written out before the tests are taken, as shown by the large figures:

$$
\begin{array}{llr}
R_2 + 1{,}000 & \ldots \quad \ldots & 1{,}060 \\
-\,(R_1 + \phantom{1{,}}100) & \ldots \quad \ldots & 135 \\
\hline
 & & 925 = C \\
2\,r + \phantom{1{,}}100 & \ldots \quad \ldots & 940 \\
\times\ R_2 & \ldots \quad \ldots & 60 \\
\hline
 & & 56{,}400 = B \\
2\,r + 1{,}000 & \ldots \quad \ldots & 1{,}840 \\
\times\ R_1 & \ldots \quad \ldots & 35 \\
\hline
 & & 64{,}400 = A \\
 & & 56{,}400 \\
\hline
 & 925) & 8{,}000\ (= 8{\cdot}6 \text{ ohms.}]
\end{array}
$$

This instrument then is practically an ordinary bridge, with a set of slide resistance coils, and is perfectly adapted for ordinary use for a Wheatstone bridge, but it has the advantage that it is also specially suited for taking the tests Mr. Mance has described. It is really an improved Wheatstone bridge, and is, I think, better suited for ordinary purposes than many of the bridges one commonly sees, on account of its having the set of single-ohm sliding-coils added to it. Mr. Mance has sent a mathematical investigation of the formula which he has adopted, in proof of its correctness, which will be given in the Journal of the Society. The formula is in fact derived from, and is in full accordance with Kirchoff's well-known laws of derived currents; so much so that any mathematician, on having his attention called to the problem, would at once work out the same formula. You get the key to

it when you remember that the electro-motive force of the earth current is represented by $e = \frac{E(R - L)}{2r + P + R l}$. You will have already gathered that Mr. Mance does not attempt to eliminate the resistance of the fault itself—he only gives us the value of the resistance caused by polarisation and by earth currents; therefore the results obtained give us the resistance of the wire at and up to the fault, but not the resistance of the fault itself. In his paper he pointedly calls attention to the importance of dealing with the interesting question of the resistance of the fault itself, and, as he remarks, it is one upon which every one has more or less experimented. He shows that its apparent resistance increases as the distance of the fault from the testing point increases, and he also shows how it varies with variations of battery power. This fact of the variation with the distance—that is to say, with the resistance interposed—would seem to point to the desirability of using as small proportionate coils as possible when testing faults with the object of ascertaining their real resistance.

TABLE I.

Resistance of Artificial Faults. Short Circuit. No. 16 Wire.

Negative Current.	Section.	$\frac{1}{16}$ in.	$\frac{1}{8}$ in.	$\frac{1}{4}$ in.	$\frac{3}{8}$ in.	$\frac{1}{2}$ in.	$\frac{3}{4}$ in.	1 in.	2 in.	3 in.
3 cells	400	235	212	190	180	170	159	147	143	135
6 „	182	102	91	82	76	72	66	64	60	59
12 „	119	57	49	41	38	36	33	32	30	29
24 „	90	41	34	28	25	23	21	20	18	17
36 „	85	37	30	24	21	19	17	16	15	14
48 „	82	36	29	23	20	18	17	16	14	12
60 „	84	34	26	21	18	17	15	14	12	11

TABLE II.

Resistance of Artificial Faults. No. $\frac{1}{16}$ Copper Wire.

Short Circuit.

Negative Current.	Sec.	$\frac{1}{16}$ in.	$\frac{1}{8}$ in.	$\frac{1}{4}$ in.	$\frac{3}{8}$ in.	$\frac{1}{2}$ in.	$\frac{3}{4}$ in.	1 in.	2 in.	3 in.	6 in.
6 cells	182	102	91	82	76	72	66	64	60	59	54
60 „	84	34	26	21	18	17	15	14	12	11	10
Difference	98	68	65	61	58	55	51	50	48	48	44
With 50 ohms in Circuit.											
6 cells	184	117	103	95	89	87	82	80	74	73	65
60 „	80	34	37	22	19	17	16	15	12	12	10
Difference	104	83	66	73	70	70	66	65	62	61	55
With 100 ohms.											
6 cells	210	134	119	113	108	103	95	92	86	85	70
60 „	90	37	30	24	22	20	19	18	16	15	14
Difference	120	97	89	89	86	83	76	74	70	70	56
With 200 ohms.											
6 cells	260	172	157	142	138	131	120	117	114	109	91
60 „	90	40	33	28	25	24	22	21	19	18	15
Difference	170	132	124	114	113	107	98	96	95	91	76
With 300 ohms.											
6 cells	300	203	185	170	160	153	145	140	135	127	110
60 „	95	45	35	30	26	26	25	24	22	20	17
Difference	205	158	150	140	134	127	120	116	113	107	93
With 400 ohms.											
6 cells	335	225	219	190	185	175	162	157	150	145	122
60 „	107	55	50	50	48	48	47	49	48	45	43
Difference	228	170	169	140	137	127	115	108	102	100	79
With 500 ohms.											
6 cells	430	320	300	275	260	247	240	230	220	208	197
60 „	120	70	66	62	60	59	58	58	57	56	53
Difference	310	250	234	213	200	188	182	172	163	152	144

Resistance of galvanometer, 144 ohms. The figures give the resistances of the fault alone without including that of the galvanometer and the extra resistances in circuit.

The earliest experiments with which I am personally acquainted in this matter were made by Mr. Preece and myself at Jersey and Southampton, in September, 1858. But more special attempts at getting at the resistance of faults were made by Mr. Laws and myself during a voyage in the Red Sea and along the southern coast of Arabia, in February, 1862. In the *Electrician* of 19th September, 1862, I published a letter on the subject, and illustrated it with the tables which are now before you, and which are interesting as touching upon the subject brought before you this evening by Mr. Mance. Those experiments were all made at sea. A length of wire was thrown overboard, and the resistance of various artificial faults measured in the sea. No. 16 copper wire was used, that being the gauge principally employed in those early days. The resistance of the fault was taken with potentials varying from three to sixty cells, and various lengths of cable were employed, and it was found that in all cases, as the battery power increased, the resistance of the fault decreased with great steadiness. A reference to the table will make this clear; and, seeing how uniformly the fact is brought out by a comparison of these results, it ought not to be a difficult matter to obtain the law and to construct a formula which would enable one to predict beforehand what would be the actual resistance of any given area of fault exposure with any given amount of battery power. In the second table another element was introduced by us, and it was one which we met with constantly in practice. Artificial faults of various sizes were tested, and uniformly varying resistances were introduced between them and the battery, and a varying battery power of from six to sixty cells was employed. The table before you shows a most surprising regularity in the rate of variation. [Instances were taken from the table in illustration and explained.] Such a mass of information as is to be obtained from a series of experiments like those must be of great interest to the mathematician who carefully considers the subject. There evidently is a law, or a set of laws, running through the figures, which could be put into the shape of formulæ and made of use to the practical electrician. A fresh series of tests should be taken on wires of the sizes now in ordinary use for submarine

cables, though even these tables can be used with advantage as a rough practical method of estimating the resistance and distance of a fault. If any one will take the time and trouble to work out from such a table a formula showing the apparent resistance of a fault under varying conditions such as occur in practice, it will prove a valuable supplement to Mr. Mance's paper, which is only deficient in that one respect. Mr. Mance's method of introducing resistances runs to some extent parallel with my own experiments.

In 1862, I also suggested, in a patent which I took out, a method of neutralising earth currents, which I think is even now not unworthy of notice. In an ordinary bridge test I inserted as a derived circuit, a battery of one or two cells, the current of which was constantly circulating around its own short circuit, and I connected the end of the cable to some part of this circuit, and moved it backwards and forwards along the wire until I found a point at which the difference of potential exactly neutralised the earth current. I was thus able, by sliding the wire along, to find a point which just brought my needle to zero, and so to eliminate the effects of polarisation and earth currents, and a measurement was then taken. The result is similar to that which is obtained by what is called "working to a false zero," which is another method of obtaining the same result. I do not know anything that gives more amusement and instruction to those engaged in submarine cable testing than the measurement and observation of faults of various kinds, especially under a magnifying glass.

Experiments can be made in a wine glass, and easily carried on on board ship. A positive current gives a somewhat whitish deposit: when the current is reversed that deposit turns into dark flakes which fall off, and a moment later the wire becomes beautifully bright. At that instant the resistance is at the lowest, and that is, I presume, the truest moment for obtaining the resistance of the fault. The changes which occur in the resistance are, however, most unexpected and instructive, and should be familiar to every electrician.

I feel that the subject of testing submarine faults has been greatly advanced by what Mr. Mance has done for us, and I hope that in this discussion we shall hear much that will be new and

useful. I also trust that his paper will induce others to experiment in the same direction, and with the same practical objects in view.

The PRESIDENT: We have been exceedingly fortunate to-night, gentlemen (and you have been more fortunate than your President, who has had those excellent diagrams behind instead of before him), not only in having Mr. Latimer Clark to put before us Mr. Mance's paper, which is so fully illustrated by these diagrams, but also in having from Mr. Latimer Clark himself a supplementary paper, containing the valuable additions which he has made, the results of many years' experience in cable work. The remarks we have heard, coupled with the paper, form an admirable basis for discussion; and, as we have plenty of time before us,—thanks to Mr. Latimer Clark, who has led us over the ground so rapidly and yet so clearly,—I hope we shall have an exceedingly good discussion.

Mr. W. H. PREECE: I have extremely little to say, but would express the very great regret that in those early days that Mr. Latimer Clark has referred to there was no Society of Telegraph-Engineers to enable the young men of those days to record the results of their very hard work. It must be remembered that the period of which Mr. Latimer Clark spoke was that between 1853 and 1863 (a period when many now present were not born), and when there were no two more earnest, more ardent, and more determined workers in this field than Mr. Latimer Clark and myself. Unfortunately for us there was then no Society of Telegraph-Engineers, there was not even a Journal, and there was no encouragement of any sort or kind to spur us on to record our results, and therefore they simply remain dormant in our memories.

I am sorry to have to confess that I see no new results in what Mr. Mance has brought before us: it is a new method of obtaining the same end. There is no electrician who has not, before he commenced his submarine cable experience, found out for himself the behaviour of artificial faults and of the ends of cables of different dimensions and of different forms, so as to enable him to obtain a thorough knowledge of the errors that faults are likely

to introduce, and to eliminate them. The accuracy with which tests are made is wonderful. Now, Mr. Latimer Clark will remember that in 1853, when the first cables were laid, the only instrument in our possession was a rough-and-ready galvanometer; and Mr. Frederick Webb, who spent so much time in repairing the North Sea cables, succeeded with the ordinary sand battery and the ordinary galvanometer in obtaining a table that gave him results of the behaviour of the No. 16 copper that then formed the core—very similar results indeed to those produced by Mr. Mance with the more elaborate apparatus that we now see before us. I am sure that in this hall there are many who have worked with instruments as rough-and-ready as those used in the old days, and who have produced corrections as true as those that Mr. Mance has produced. The errors due to earth currents and currents of polarisation are very fully dealt with in "Culley's Handbook."

Well, the moral that I draw is, how fortunate the young men of the present day are in having a Society like this to receive the records of their experiments, and also how fortunate we older men are to see history repeating itself. As I said, I see nothing new in fact, though very novel in method, but I see something very nice—I see that Mr. Mance in the far distant East is bringing to bear on these electrical problems scientific attainments of a very high order; and we are singularly fortunate in having men in other parts of the world who are bringing to bear the results of magnificent constructive skill in England, the results of professional training at home, and the results of scientific thought; and I hope that those who are here will take the lesson to heart, and that if they do go abroad they will solace their spare hours in hot climates by cooling us with observations, though sometimes they may be a repetition of old facts in a new form.

Professor W. E. AYRTON: I am afraid that Mr. Preece has hardly done proper justice to Mr. Mance, for had I spoken before Mr. Preece I should have said that there was a good deal of novelty in Mr. Mance's communication. It is necessary to carefully distinguish between the new mode of testing communicated by Mr. Mance and the results which Mr. Latimer Clark has brought forward this evening in explanation of some experiments he himself

made many years ago, by methods altogether different to that adopted by Mr. Mance. It may be pleading ignorance to say that the communication of Mr. Mance this evening strikes me as quite novel; but I certainly had not seen this method of eliminating the effect of an earth current, and must confess my ignorance in that respect. The moment the method is suggested one wonders why one did not see it before, and one is only too glad, for the reputation of telegraph-engineers, to hear that it has been known to Mr. Mance for four years. Hitherto the ordinary way of eliminating the effect of an earth current, or polarisation current, in a submarine cable has been by reversing the testing battery. For land lines there is a better method than that, but as it is not applicable to submarine cables it does not affect the credit of Mr. Mance's method. In a land line the simplest plan is to use the bridge key in a slightly different way to that in which it is ordinarily employed. The bridge key of course first puts on a current to the bridge, and then puts on the galvanometer. If you reverse the operation,—first put on the galvanometer without the battery, and then put on the battery,—you can get a test, with a land line, quite independent of the polarisation current. What is done is, first, to put the land line on to the bridge, and the effect of the natural current at once deflects the needle; then, without putting on the testing battery at all, the controlling magnet is altered until the needle is brought back to zero; the testing battery is then put on, and the test taken from that artificial or false zero. It is well, when the testing battery is taken off, to see that the natural current on the line has not altered. This method of a false zero is not applicable to a submarine cable on account of the charge which is given to the cable by the testing battery; and therefore with a submarine cable it is necessary first to put the testing battery on to the bridge and cable before the galvanometer is put on, and so it is not easy to use the false zero method employed on a land line.

From Kirchoff's laws we find that, when balance is established, the unknown resistance is a function of all the other resistances in the circuit and of the ratio of the unknown electro-motive force to the electro-motive force of the testing battery. To obtain a

second equation, so as to make it possible to eliminate this ratio, it is usual to make a second test after reversing the battery; and it does not seem to have occurred to any one, until Mr. Mance suggested it in his paper of to-night, that a second equation could be obtained by altering the values of the two proportional coils.

It is not quite right to say that the current passing through the fault in Mr. Mance's test is not affected by altering the proportional coils: it is affected by altering the proportional coils if you alter both of them. But I think that you will be able to get the second equation, and so eliminate the ratio of the two electro-motive forces, by merely altering one of the proportional coils; and, if this be the one not adjacent to the unknown resistance, the current passing through the fault would be very slightly changed. Possibly Mr. Mance has tried that plan and finds his own better; but, if it has not been tried, I think it may be found to be a slight improvement on the method he has worked out.

Another plan that I would propose would be simply to change the resistance in the battery circuit, but this would of course alter the current passing through the fault very materially. It would, however, have one of the advantages of the other method I above suggested, viz., that one thing, and not two, would have to be altered before making the second test; and hence the second test could be made to more immediately follow the first, and there would be less chance of the resistance of the fault altering.

In regard to the second part of Mr. Mance's paper, the determination of the resistance of a fault, and which is quite distinct from the first part, some years ago Professor Perry and I made a communication to the Society on that point, and we explained a method which depended, not on what the resistance of the line was when the positive current was sent, or what the resistance was when the negative current was on, but which depended on the rate at which the resistance changed when either current was flowing. We showed that if you took a copper plate and a copper wire, dipped them into a vessel containing dilute acid, and sent a positive current with one, two, or three cells from the plate to the wire, the current increased with time, or the apparent resistance

diminished; whereas, if the current went from the wire to the plate, the current diminished with time, or the apparent resistance increased. Now a small fault will act like the exposed wire in our experiment; whereas, if the resistance of the fault be small, or much of the conductor be exposed, the result will be as if the current were passing between two plates in the liquid.

Hence it seemed to us possible, by such experiments on the rate of alteration of the apparent resistance first observed with one current, and then with the current reversed, to determine with a certain amount of accuracy the resistance of the fault.

Mr. A. E. KENNELLY: Mr. Preece has informed us that the general outline of the way in which Mr. Mance arrives at the resistance of the fault, or rather the way in which he arrives at the quantity of resistance due to polarisation, is by no means new in character; but I should fancy that what we have to thank Mr. Mance for particularly is, rather, that he has given us a particular formula which we are at liberty to criticise or to confirm. Suppose that we had a battery of no resistance, we find this a very simple formula, expressed by multiplying the smaller reading by 1,000, subtracting from that the product of the larger reading multiplied by 100, and dividing that difference by the difference of the two readings plus 900 ohms. I fancy, and there are many here who can correct me if I am wrong, that this savours rather of the empirical; and I should think that it would have been better, if possible, to let us have the whole formula, not in terms of two different forks of a bridge, but rather in those of the differences of currents which those two forks entail in passing through a fault.

It has long been known, judging by text-books on the subject, that the apparent resistance of a fault was some function of the quantity of current passing through it; and the late Mr. Schwendler drew special attention to that point in the tests which he has so clearly shown in his text-book on the subject. The question remains, what function? There is, I fancy, a special disadvantage in dealing with two different forks, because, supposing that the resistances of a fault, and the line up to the fault, are so great as to require for sensitiveness a fork in the bridge of

1,000 to 1,000, then it would necessarily follow that to diminish the balance to 100 to 100 means loss of sensitiveness in ascertaining what the resistance in circuit then is; and if, *vice versa*, you are so close to a fault that 100 to 100 is the best fork, then it is clearly objectionable, as Mr. Mance states, to increase it to 1,000 to 1,000. Surely, then, it would have been better to let us have, in Mr. Mance's own formula, the ratio in which the resistance of a fault and polarisation increases with different current strengths, for that is simply the basis, as I read it, of the present paper.

In judging of the efficiency of the tests placed before us this evening, by the difference which exists between the tests uncorrected and those corrected according to Mr. Mance's method, I see that in the example 2 before us the uncorrected test placed the fault at 138 ohms distant, while the corrected test placed the fault still further, 172 ohms. I would like to be informed how it happens that the uncorrected tests placed the fault nearer the testing station than the corrected tests, because I should have thought that the uncorrected test to earth current zero would have put the fault further off than Mr. Mance's better method. If Mr. Mance tested in that instance from the end which gave 138 ohms, I cannot at present see how a lower result is obtained by an uncorrected, than by a corrected method.

Another point in the paper, and one for which I think we are much indebted to Mr. Mance, is where he mentions that when he has found in practice the copper wire touching the external wires of the cable, the resistance of the fault is not reduced to zero; in fact, he has not found dead earth. I should like to know how this was, unless there existed a coating of rust between the copper and the iron wire, or a coating of some compound of copper; because I am informed, on good authority, that in brass-taped cables, when at a fault the brass tape touches the copper, the resistance of the fault has been zero within the limits of observation.

Mr. LATIMER CLARK: I must express my regret that among so many practical cable layers and repairers we have not had the advantage of a longer discussion. The paper dealt with a practical question, but has been treated rather from a mathematical point

of view; and, seeing the number of experienced electricians who are present, I should have hoped to have had a more instructive discussion on the subject.

In reference to Mr. Preece's remarks, I very much agree with what Professor Ayrton has said, and he has said it much better than I could have done. I do feel that we are indebted to Mr. Mance for giving us a formula, instead of leaving us to rely on personal skill and judgment. There is no denying the fact, that by practice and experience the testing is in these days done admirably on board our cable ships, but much of it is what may be called, without disparagement, rule-of-thumb testing; and I think it is always a great advantage, wherever possible, to have a formula which even a novice can apply, and to have definite numerical values which can be communicated from a distance, rather than to be dependent upon the personal skill and judgment of the persons on the spot. There will always remain ample room for the exercise of these qualifications when science has said its last word.

Although I have had great satisfaction in bringing this paper before the Society on behalf of Mr. Mance, I do not feel myself competent to criticise it from a practical point of view, or to answer the objections to it. I have thrown out various suggestions, rather in the hope that they would be taken up by those practically engaged in such work, and with the object of promoting discussion.

I have nothing to add, except to say once more that I feel that Mr. Mance has brought before us a very useful and distinctly practical improvement in the method of testing faults, and one which I feel confident will be found of considerable value.

The President: I have very great pleasure in calling upon you formally to accord your vote of thanks to Mr. Mance for his very interesting paper, and also to Mr. Latimer Clark for his communications this evening, and for putting Mr. Mance's paper so well before us. There must have been a great deal of trouble and care spent in preparing all the tables and diagrams in order to put the whole matter before us in so clear a light. In fact the power of discussion seems to have been almost taken away,

in consequence of the very complete manner in which Mr. Latimer Clark has placed the whole subject before us.

The vote was put, and heartily awarded to Mr. Mance and Mr. Latimer Clark.

A ballot then took place, at which the following were elected :—

Foreign Member:

Barbosa, Don José Casas.

Member:

Stearn, C. H.

Associates:

Brunton, S. B.	Stevenson, J. S.
Chamen, W. Ashcombe.	Willans, P. W.
Oakley, E. M.	Williams, M. J. M.

Wilder, F. L.

Student:

Bowen, W. A.

The meeting then adjourned until 22nd May, 1884.

The One Hundred and Thirty-sixth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 22nd May, 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

Mr. J. W. Schwafe was nominated as Associate, and his name was ordered to be suspended.

The PRESIDENT: It is customary at the last meeting in May to ballot for any new members who may have been announced on that evening, and with your permission I propose that the practice be continued this evening, and that the ballot for Mr. Schwafe take place before the meeting adjourns.

Agreed.

Donations to the Library were announced as having been received from the Physical Society, M. Blavier, Mr. Munro and Mr. A. Jamieson, and Mr. H. Bramhall, to all of whom a hearty vote of thanks was accorded.

The PRESIDENT: At our last meeting I made allusion to the arrangements which were then in progress with regard to a Conference of the Society to be held at the Health Exhibition. I have now to state that those arrangements have been carried out in accordance with suggestions made by the Council, and the date fixed for the Conference of the Society is July 3rd. The Conference will take place at the Health Exhibition, and papers will be read and discussed on that occasion. One paper will be on "Electric Lighting in Relation to Health," by Mr. R. E. Crompton; and a second paper will be on "The Application of Electricity to Medical Purposes," by Dr. W. H. Stone. It is proposed that the Conference shall open at eleven a.m. on the 3rd July; that the first paper shall be read and discussion upon it taken; then, after the luncheon adjournment (about half-past one or two o'clock), a second meeting will take place at two or three o'clock (as may be arranged), when the second paper will be read and the discussion upon it taken. It is thought that by carrying out this plan the Conference will be got through during

the day. Due notice will be given, and tickets for admission to the Exhibition on the occasion will be posted to members.

The following paper was then delivered:—

ON THE ELECTRICAL CONGRESSES OF PARIS.

By W. H. Preece, F.R.S.

Mr. President and gentlemen,—The year 1881 marks an epoch in the history of electricity. In that year the French Government, with a liberality that fully justifies the praise that has been bestowed upon them, contributed 300,000 francs for the purpose of holding a Congress and instituting an Electrical Exhibition in Paris. The object of that Congress was said to be to codify electrical science, and to sound its depths; and there is no doubt whatever that, in the conclusions to which that Congress led, that wish has been thoroughly carried out.

In 1881, no less than 293 of the electricians and scientific men of this world met together in Paris. Of that number, France contributed 70, England 38, Belgium 32, Germany 15, and other countries in proportion. I think that amongst those who assembled there, there was scarcely one—if there was one—single shining light in the science of electricity found absent. There we had the brightest stars, and chief among them was our own Sir William Thomson. There we had Helmholtz, Clausius, Kirchoff, Wiedemann, Rowland (from America), Dumas, Lenz, Du Bois, Raymond, and every single man who has appeared for many years past at the head of papers that have been published in our scientific journals upon the progress of electricity. One of the most marked features of the 1881 Congress, and of the subsequent congresses held in Paris, was the admirable organisation with which the proceedings were conducted—an organisation for which the French nation is remarkably distinguished; but the mode in which that organisation was carried out was chiefly attributable to the skill, to the decision, and to the energy with which the President of each Congress—M. Cochery, the Minister of Posts and Telegraphs—conducted the proceedings. The Vice-Presidents were six in number, three foreign members and three French members. The Vice-Presidents on the foreign side were

Sir William Thomson, Professor Helmholtz, and Professor Govi; on the French side there were MM. Jules Ferry (the present Prime Minister of France), Sadi Carnot, and the great Dumas, who has since, unfortunately, passed away. This Congress was divided into three distinct and well-marked sections. The first section was devoted entirely to the discussion of electrical units and the determination of their form. The second section was devoted to the consideration and discussion of questions that affected telegraphy, telephony, and the application of electricity to railway purposes. The third section was rather more discursive in its character. It dealt with light in its various branches, with electro-physiology, with lightning, with the transmission of power, with clocks, with meteorology, and with various other questions aside from telegraphy and the more marked features of the practical applications of electricity.

The meetings were really divided into three classes also. We had first a meeting of the full Conference, where the general subjects were discussed, and where the conclusions and resolutions arrived at by the sections were passed; secondly, we had sections to which different members attached themselves; and, thirdly, we had lectures given by a few of the members. The hours of meeting were so arranged that none of the sections ever clashed with each other, so that everybody had the opportunity of attending every meeting that was held.

The first meeting in Paris, in 1881, commenced on September 15th, and closed on October 5th. Eight full Conferences were held, and a great many minor sectional meetings took place which I need not now enumerate. I will not detain you in giving the results of this Congress, though I have much to say on the subject; but in reality this first Congress proved to be a grand opportunity for everybody who had anything to say, or who had any thought whatever, to let off a great deal of steam. There was a great deal of this steam let off, and a great deal of time was spent in talk, but the result was certainly extremely beneficial, and the real hard work was done rather at subsequent Conferences than at this first one.

Now, the first great result attained at this first Congress of

1881 was the acceptance of that magnificent system of units of measurement that will always last as a standing memorial to the honour of the British Association. The system of units known as the C.G.S. system, based upon the centimètre, the gramme, and the second, was accepted by the Congress at Paris, and by that means it became international as well as universal.

The second result was that the Congress defined what should be the ohm (I will not give you that definition now, for I shall have to lead to it again directly); it defined and accepted the ampère; it defined and accepted the coulomb; it defined and accepted the farad: so that, with the smallest possible modification, it established that system of electrical units that is now familiar in our mouths as household words. But inasmuch as they could not accept and define without absolutely determining what these units were to be, it was decided that a future commission should be held in Paris, where the absolute determination of the ohm should be decided upon; where some system could also be decided upon by which we could secure regular observations on earth currents and atmospheric electricity; and also where it could affirm and determine some definite standard of light. The only material resolution, not subsequently discussed, that was decided at that first Congress was, that a millimètre gauge should be used for determining the size of wires, which, I am sorry to say, owing to the very great obstruction that exists in England to the introduction of the metrical system, has not yet received any attention amongst us. Without coming to any definite conclusions or results, there were several subjects discussed, and very fully discussed, for instance, electric lighting. Many meetings were held where a battle royal took place between two, if not three, distinguished exponents in France for the transmission of power.

Again, we had discussions on the best arrangement for clocks and chronographs, and for the application of electricity for such purposes. Meteorology, medical electricity, and telephone working also occupied much attention; but the real fact was that we were too many on that occasion for real sound work; and although much good was done much more remained to be done, and that

which remained to be done was done at the subsequent Conferences that were held.

Well, the Congress of 1881 passed, and was followed by a second Conference, held in Paris in 1882. In October, 1882, a smaller Congress met there. There were only forty-five members present, and of those forty-five we in England were only represented by one; but our one was a host in himself, and quite sufficient to represent us all, for our one on that occasion was again Sir William Thomson. At this second Conference, again the divisions were three. At the first commission, as it was called, units were considered; at the second commission, atmospheric electricity and earth currents were discussed; and in the third commission the subject discussed was a proper standard of light. Again they had to part without coming to definite decisions. Much work had been done; much knowledge was produced and described at that meeting; but they parted without any absolutely definite conclusion being come to, although the results, which will be printed in full as an appendix to my remarks to-night, deal very clearly with the points under discussion. But *the* grand point that this Conference met to discuss was, in reality, what should be the ohm. It was found that only one observer had devoted very much time to this enquiry, and that was Lord Rayleigh; and although it was felt by every one that his results were so exact and his powers of observation so great that the utmost confidence could be felt in his figures, nevertheless it was thought that, before such a momentous question was decided as the length of the column of mercury representing the ohm, other observers should take up the question; and, as there were five distinct methods by which the ohm could be determined, it was agreed that the meeting should be postponed for another twelve months for further observations to be made.

Again, they passed resolutions that observations should be made on atmospheric electricity, on the behaviour of lightning-conductors, on earth currents, and on the question of a standard of light. But, as I say, the definite conclusions were really left for the subsequent meeting that took place this year, when fifty of us met in Paris on April 28th, and were again marshalled, under

the able presidency of M. Cochery, into the same three sections. We were received by the Prime Minister of France, who gave us a speech of considerable eloquence. But on this occasion we were more numerous in being represented, for we had there not only Sir William Thomson, but we had our President (Professor W. G. Adams), Professor Carey Foster, Mr. E. Graves, Professor Fleeming Jenkin, Professor Hughes, Captain Abney, and myself.

I believe we did our work well. I believe that not only were the interests of English science and telegraph-engineers well represented there, but I believe that, taking the Congress as a whole, the work was well done, the conclusions come to just, and the result must be nothing but beneficial to the workers in electrical science.

Having passed briefly in this way through the history of what has been done, I now propose to take up *seriatim* the main subjects that were discussed, and upon which the results were based.

Of course you all know that the chief point that occupied our attention—the chief point which we met to determine—was the length of a column of mercury of one square millimètre section that should represent the ohm. I told you that at the first Conference it was decided that the electrical units of the future should be based on the C.G.S. system, and in order to accept that universally it was necessary that we should at least have some definite understanding as to what the ohm was. There may be some here who would be inclined to ask the question, What is the use of troubling oneself so much about the length in mercury of that which is called the ohm? But we must remember this, that a means of accurate measurement is simply a proof of accurate knowledge. We only know a thing properly, we only know the practical application of any scientific fact, when we are able to measure it accurately; and we can only make practical use of a formula (for instance, Ohm's law) when we are able to give the proper numerical value for the letters that are used to represent the law.

A formula, as a formula, is simply an expression of a physical law, and it only becomes of any use whatever when you are able

to represent the letters involved in that formula by their proper numerical values in a proper coherent system of measurement. All the exactness that is so essential in science, all the exactness that is so essential in carrying out the practical details of electric lighting, of telegraphy, and all other matters, can only be obtained when we possess absolute and accurate means of measuring.

Now all practical systems of dynamical measurement are really based or are dependent upon a proper conception and a proper knowledge of energy and its applications. There is no grander term, there is no term of grander generality in this world, than that which we call "energy." Energy is the basis of all that moves, it is the base of all that lives. As Shakespeare says, "the stars that sing in their motions," the invisible play of the molecules (which our friend Professor Hughes deals with so much), are all in their movements and in their play due to the presence of this so-called energy. Energy is measured by the work done—in other words, by the mass put in motion, and the distance through which it is moved. The rate of doing this work—and this is a point that I want to particularly urge to-night—is power; and this is measured by the time in which this work is done. Hence, in the measurement of energy, we want to know three things—we want to know the mass moved; we want to know the distance through which that mass is moved; and we want to know the time that it has taken to move that mass through that distance. Thus, in all systems for the measurement of energy, we should have units of mass, of length, and of time; and the units that are taken for that purpose are, the centimètre for length, the gramme for mass, and the second for time, and hence it is that the system is called the centimètre-gramme-second (C.G.S.) system. Now what I say to you of energy is true for all its forms, for all properties of matter, for all kinds of motion; and it is thus true for heat, for light, for electricity, for magnetism, and for all other mere forms of energy: so that, if we establish a system which enables us to measure all forms of energy in one coherent, connected system, the correlation of all the physical forces is established by a definite system of measurement applicable to

them all. There is nothing that has so far established the doctrine of energy as the establishment of this system of so-called absolute measurement. Let us glance for a minute or two at what has been done in this direction. The whole system originated with Gauss in the year 1832, and Gauss and Weber together worked it out, and were really the originators of the present so-called absolute system. But there was one quiet, retired, scarcely-known man at the time whose work in this field was simply wonderful, whose works have just been published by the Physical Society, and that was Joule. As far back as 1841, Joule, in his quiet retreat in the neighbourhood of Manchester, possessed and used a knowledge of measurement and a knowledge of electrical units that would even grace our principal electricians of the present day. He established a unit of resistance, he established a unit of current; and it is a very curious fact that the unit of current that he established does not depart very much from that which has been accepted by the Paris Congress. His idea of unit current was, that current which would decompose a chemical equivalent in one hour; and, as he measured the chemical equivalents in grains, so he took that current which would decompose nine grains of water in an hour, and nine grains of water are decomposed in one hour by very nearly two ampères, in fact by 1·8 ampères: so that Joule's unit of current that he *used*, and used throughout all his papers over forty years ago, was very closely allied to the unit of current that was adopted this year at Paris. The marked features in Joule's experiments have been summed up in two words by Sir William Thomson — he calls Joule's investigations in this field as distinguished by "magical accuracy;" and it is perfectly astounding how Joule, by his methods, was able to check and correct the observations made by our British Association Committee. Well, we go on now for twenty years before much was done, and in 1861 there was a paper read before the British Association by two friends of ours who are now sitting at this table—Sir Charles Bright and Mr. Latimer Clark. They read a paper, and proposed a system of measurement, and gave a nomenclature to this system which is the very nomenclature that we use now. The names "volt," "ohm" (or "ohmad"), "farad,"

"weber," and one or two others, were introduced and have been accepted. The joint paper of Mr. Latimer Clark and Sir Charles Bright attracted much attention, and especially that of Sir William Thomson, who in that year of 1861 obtained the first Committee of the British Association to take up this question; and from 1861 to 1869 there never was a committee in this world that did its work so thoroughly and so well. Their reports are a monument of work and accuracy, and they have tended more than anything else to establish the science of electricity in this country upon the sound footing that it possesses at the present moment.

But there was another worker in the field in Germany, and that was Dr. Werner Siemens, who had established a unit that had the power of reproduction which nothing we had in England possessed; and the result has been that, while the Paris Congress did not accept the Siemens unit *per se*, it did accept Dr. Werner Siemens' mode of forming and reproducing that unit.

These units may be either arbitrary, such as the foot. Who knows what a foot or an inch is? There is nothing more meaningless in this world than an inch, and no one can tell you what is an inch except that it is "three grains of wheat put in a row;" and our system of measurement that we submit to with such foolish pertinacity, and about which so much conservatism is shown, is simply based upon the size of a grain of wheat that may be found in some particular part of an ear of corn. And this system is established by Act of Parliament! It is this system of measurement that controls the enormous commerce of this country. The country's manufactures are based upon such a ridiculous, little, foolish thing as a grain of corn.

Well, the mètre is not much better; for the mètre itself is based upon a supposed measurement of a quadrant of the earth's surface, and we know that it is almost as wrong as the inch. At any rate it is certainly wrong, but nevertheless there it is. It is an arbitrary unit; it is fixed; it is determined by certain masses of metal; it can be reproduced at any time; and it is essentially what I call an arbitrary unit.

The "second" is to a certain extent arbitrary, nevertheless it

is universally used; and I do not want to shake your faith or your confidence in the second, as I hope I have succeeded in doing as regards the accuracy of the inch.

Now the gramme is different; the gramme is based on the mètre, and I want now just to draw a distinction between the metrical system and the decimal system. Mind you, this is a fight that I am fighting nearly every day. There are friends of mine whom I meet sometimes in this room, in connection with another institution, who are deadly opposed to the introduction of the metrical system; and when you talk to them upon the metrical system, the reasons they bring against it are reasons against the decimal system; and if you talk to a man about the decimal system his reasons are those which he urges against the metrical system. The two systems are entirely distinct: the metrical system is based upon the mètre; the decimal system is an arithmetical and mathematical system based upon the powers of 10. I believe that the very first man who really proposed a decimal system as applied to scientific measurement was our great Watt himself; and in his "Life" there is a most wonderful letter, written by him more than a hundred years ago, where he recommended the introduction of a decimal system into England. We could use a decimal system, based upon the inch, just as we could use a decimal system based upon the mètre; but when those who use arguments against the mètre state reasons which are applicable to the decimal system, *do* point out to them that the two things have nothing whatever to do with each other. Now the objection to the metrical system is really due to the fact that we carry in our pockets a foot-rule. If we could only gradually induce our boys to discard their foot-rules, and could let them gradually obtain a mental conception of parts of a mètre, then I believe our grandsons would talk to each other in nothing but millimètres and mètres, rather than in thousandths of an inch, or "mils," as they are foolishly called. There is nothing like practice to effect changes. What is most wonderful is the marvellous rapidity with which electrical measurements have been introduced in this country: we changed from the weber to the ampère without the slightest hesitation. There

is not a man in this room who felt the slightest inconvenience from the change of name. It was even proposed at the one time to enlarge the unit of electro-motive force ten times. Unfortunately it was not carried out; if it had been the change would have produced no inconvenience whatever; but we have introduced at the same time that which I may just briefly run through. There is not a workman in our employ, there is not a workman about us now, who is not as familiar with the ampère, with the farad, with the volt, and with the ohm, as he is with a pint of beer or with an inch.

Now what was the method adopted by that British Association Committee in 1864 of which I have already spoken? Their great work was the determination of the practical unit of electrical resistance. They used a circular coil of wire rotating in the field of magnetic force of the earth, and the deflection of a needle suspended in the centre of that coil was measured, the conditions were carefully recorded, and the result was the ohm of 1864. Although the greatest possible pains were taken, and every possible point of error carefully examined, nevertheless there is no doubt whatever that some error crept in, and the ohm, according to the measurements of the British Association Committee, turned out to be a little more than 1 per cent. wrong.

There are several means of obtaining this measurement. There is the method of the British Association, based on Weber's original plan, that I shall allude to presently, and which was worked out by Sir William Thomson. There is Weber's plan, where, instead of the coil rotating constantly in the field of the terrestrial magnet, it made a half revolution, it turned through 180°, and the momentary current produced was measured. There is also the plan due to Kirchoff, who measured the current of induction—the current induced in a secondary wire from a current passing through the primary wire in its neighbourhood. Then there is the system of Lorenz, which depends upon the velocity of rotation of a brass disc in the interior of a large solenoid traversed by a constant current. There is also the second plan of Weber, who measured the damping effect upon the vibrations of a magnet in the neighbourhood of a wire carrying the current;

and lastly, there is the thermo-electrical effect due to Joule. All these measurements and modes were carefully considered by the Congress in Paris. They were all collected and tabulated, and it was found that the mean of the observations showed that the length of a column of mercury of one square millimètre section, which represented the ohm, was 106·02 centimètres. Lord Rayleigh's measurement was 106·28; Mascart's, 106·32; Kirchoff's, 106·33. But there were several members of the Congress who desired to bring the length lower,—they were perhaps the noisiest portion of the Congress,—and the result was that, to get a settlement, they succeeded in bringing the majority to their way of thinking, and more, perhaps, for the reason that a round number is a very pleasant thing to handle. It was also most desirable that there should be unanimity of opinion; and it was agreed that the ohm should be represented by a column of mercury one square millimètre in section and 106 centimètres in length. That is not, however, the true ohm. Draw a distinction between the true ohm, the Congress ohm, Lord Rayleigh ohm, and the British Association ohm. We shall use nothing from now but the Congress ohm, although in scientific calculations of great accuracy it will be certainly necessary to adopt a correction of perhaps 1 in 500 to bring the results mathematically right.

The resolutions arrived at by the Congress were as follows:—

1. "The legal ohm is the resistance of a column of mercury of a square millimètre cross-section and 106 centimètres in length, at the temperature of melting ice."

The word "legal" ohm is a very bad word, but no better was proposed, and it was accepted.

2. "The Conference expresses the wish that the French Government should transmit this resolution to the States, and recommend the international adoption of it."

3. "The Conference recommends the construction of primary standards in mercury, conformable to the resolution previously adopted, and the concurrent employment of scales of secondary resistances in solid alloys which shall be frequently compared amongst one another, and with the primary standard."

I was in great hopes that one of these primary standards

would have been here to-night. Professor Mascart promised that I should have one, but being of glass, and of rather delicate structure, I suppose he did not like to trust it (and he was quite right) to the tender mercies of our railway companies. But he himself will bring one over, and I believe we may count upon having one presented to our Society.*

4. "The ampère is the current the absolute value of which is 10^{-1} in electro-magnetic units."

5. "The volt is the electro-motive force which maintains a current of one ampère in a conductor the resistance of which is one legal ohm."

This Congress ohm shows that the old British Association unit of 1864 is really ·9887 of this new ohm, or, in other words, this new Congress or legal ohm is equal to 1·0114 of the British Association unit, so that all of us who have old boxes marked with the old ohms have simply to multiply those figures by that coefficient of 1·0114. But it happens that the increment due to temperature is such that our present coils, if adjusted to the old ohm at 32° F., will be true for the new ohm at 58° F., and it is only necessary to bear that in mind when carrying out really accurate scientific measurement. We can still go on using our present coils: all our cables, all our wires, have their resistances recorded in the old ohms: as long as we use the present boxes with the old measurements, we shall not make any serious mistake; but in future there is no doubt that all new coils will and must be made to this new standard, and we must employ this coefficient to bring our measurements down to absolute accuracy. So much for units.

But there is just one point in connection with work and energy. There is one unit that we use in England, of immense value and of immense service, that has come into use like a flash of lightning, and nobody who works in electric lighting or in the transmission of power can help using it, and that is the "watt." Now I want you to notice this, and that is the reason why I made the remark before, that there is a difference between work

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* It arrived the next morning.

done and the rate of doing work. The rate of doing work (Sir William Thomson calls it "activity") is generally known as horse-power. Now the work done by an ampère through an ohm in one second is a watt, and a horse-power is equal to 746 watts. "Horse-power" is as ridiculous as our "inch:" it means nothing, it has crept into use by practice, or sheerly from the want of something else; and I am bound to confess that I look forward to the time when the term horse-power will be looked upon as a thing of the past, and when we shall speak of the power of engines, as we now do of the power expended in circuits, by so many watts. In fact, except for the word, a kilowatt is a very much better unit than a horse-power. A kilowatt is the Board of Trade unit as adopted in the Electric Lighting Act: a thousand volt-ampères per hour is a kilowatt. A kilowatt ought really to replace horse-power. In fact, at the Paris Congress of 1881 I proposed that this very unit should be called a watt, but the next year our old lamented friend Sir William Siemens proposed that the smaller unit should be called a watt, and it has been accepted. It is a most useful term, but it did not go down with our friends in Paris. We were anxious to propose it, but there was strong objection to it, and for very strange reasons: one was because there is no "w" in the French language; another was that there were already too many names, and they thought it would be very much better if we brought the term into existence in England, and then it would gradually creep into use over the whole world—a very delicate and a very nice compliment to us.

Now, gentlemen, the objects attained with regard to the question of units were, firstly, the system of units introduced was based on something invariable: the mètre, although wrong, is invariable; the gramme and second are equally invariable.

Secondly, the system has become so universal that there is no necessity for using those bothering and vexatious dimensional formulæ with which some of our books are filled. There are no constants required in the calculations: the whole system is connected together in one coherent whole, and, with a little thought, it becomes within the comprehension of all. Moreover,

it is most easily introduced, and gives great exactness. It is independent of language, it is independent of locality, and it has the great advantage that it is the thin end of the wedge to introduce the metrical system into England. You may depend upon it that we electricians, sooner or later, will force the use of the metrical system into commerce and into manufacture.

The second Commission dealt principally with atmospheric electricity. Well, they have not done so much in this direction on the Continent as we perhaps have done here. We held a Conference in England some three or four years ago, and I doubt whether there was ever a Conference that was so violently abused as that Lightning-Rod Conference that took so much pains and trouble to write a report. Nevertheless, that report contains one of the finest collections of facts in regard to lightning and atmospheric electricity that is to be found in this world at the present time.

The resolution passed by the Congress was:—"It is desired that the results of observations collected by the various administrations be sent each year to the International Bureau of Telegraphic Administration at Berne, which will make a digest of them and communicate it to the various Governments."

At Berne there is a central office maintained by every civilised administration. It is a kind of neutral ground where everything is collected. Statistics are examined there and distributed to all the different administrations existing; and it was thought that no better place could be found for collecting and digesting the records of thunderstorms, the records of strokes of lightning, and the records of the behaviour of lightning protectors, than at Berne. It has been decided that returns on a uniform basis shall be collected and sent annually to Berne. The particular form has not yet been absolutely decided upon, but, as soon as it is, it will be printed, and I hope that a specimen will be printed with the appendix to my remarks this evening.

The next point that the Congress dealt with was that of earth currents, and the Committee passed the following resolution:—"That the Conference expresses the wish that observations of earth currents be pursued in all countries."

The one thing of all that certainly surprised me very much at the meetings of this Congress was the apparent—I can call it nothing else—ignorance that was displayed by other administrations on the behaviour of these peculiar visitors. It happens that, twenty-nine years ago, my friend Mr. E. Graves edited a small paper called *Our Magazine*, and the very first scientific paper that I ever wrote in my life was in the year 1855 for that journal, and was on what we then called "Deflections." I have read that paper since with some care, and I am astonished to find how little has been learned from 1855 to the present day in the behaviour and quality of these peculiar currents. They are always present on all telegraph lines; they vary in direction, they vary in strength. As a rule they are very slight; they have a daily rise and fall, so much so that we have had one or two papers here from Mr. A. J. S. Adams, who has pointed out that there seems to be a "tide in the affairs" of these electric currents. They have an annual variation, and they follow that strange period of eleven years that is accompanied by sun-spots, by famine, by the potato disease, and a few things of that kind. Every now and then we have abnormal storms—we have sudden disturbances taking place somewhere that produce enormous currents of electricity flying through our wires and entirely stopping all telegraphic working. Last year we measured some of these disturbances where the currents approached 100 milliampères: the ordinary working current rarely exceeds 25 milliampères: so that on such occasions the earth current was four times that of the ordinary telegraphic working current. We know that electromotive force varies with the length of the wire; we know that these currents vary exactly with the variation of magnetic elements—they are coincident with the aurora borealis and with certain peculiar atmospheric effects that produce increased scintillation of stars. We have certain currents always accompanying earthquakes. In fact the literature on this subject is very great indeed; and Mr. A. J. Frost, our Librarian, has published in our proceedings a catalogue of the various works that have been printed in England on this subject. At Paris there was an extremely interesting and able paper by one of the oldest telegraphists

living, M. Blavier, who is the head of the Technical School in Paris (whose first work on telegraphy was printed, I think, before the majority in this room were born—I read that book over thirty years ago), and this M. Blavier has collected together and carried out in France a system of observation by automatic photographic records that are extremely interesting. His book has been presented to the Library of this Society, and is within the reach of all. Our friends on the Continent are gradually acquiring a knowledge of these earth currents, and, when the suggested returns are sent to Berne, we shall in the course of time know a little more than we do now. We shall probably then know what we want to know—the influence that the sun has upon our earth in producing these currents; or, rather, we want to know the connection between these earth currents and the movements of the sun.

The last subject dealt with was the standard of light, and the resolution passed by the Congress was: "That the unit of each kind of simple light is the quantity of light of the same kind emitted in a normal direction by a square centimètre of surface of molten platinum at the temperature of solidification."

The practical unit of light is the quantity of light emitted normally by the same source; so that, if we are to follow the Congress of Paris on this point, we are to accept as a standard of light the light emitted by a given surface of molten platinum.

We were shown this standard in Paris by its author, M. Violle, but what we really saw was a mode by which he measured the luminosity of the molten surface of platinum; but, to my idea, it was no standard of light at all, and I must confess that I am not at all pleased to have to bring back to England a standard that nobody can reproduce, and nobody can use. However, the Congress, in their wisdom, decided that this was to be the standard in future, and I wish them joy of it; at any rate, I do not think we shall use it in England. Unfortunately, the Committee of the British Association, acting principally under the guidance of Captain Abney, was not quite prepared with a unit. M. Violle's unit was the only one before the Congress, and they were obliged to accept it; but they would have acted much

more wisely if they had postponed its consideration until one or two other units had been proposed and tried, as in the case of the ohm.

Captain Abney and I have been independently working for a very long time in the direction of making an incandescent, or glow lamp, a standard of light. Captain Abney has now before the Royal Society a paper that will be most valuable in this respect. It is perfectly possible to reproduce any light you want with an incandescent lamp, with a constancy and invariability that is simply marvellous. You can produce a light in an incandescent lamp matching in quality exactly what you want to measure. If you want to measure an ordinary candle, you can do so; a gas lamp, ditto; or, if you want to measure an arc lamp, you can even produce it by an incandescent lamp properly manipulated. By an incandescent lamp the light can be reproduced anywhere, and it also has the enormous advantage that the light emitted by a glow lamp is directly connected with the watts expended in the lamp; and so the light emitted by a glow lamp is directly connected with our system of electrical standards based upon the C.G.S. system. I feel convinced that when Captain Abney's work in this direction is put before us, and when we see exactly how we can reproduce it, there is not a man in this room who will not use the Abney standard—or, rather, we should call it the British Association standard, and give it a name—in preference to this Congress standard. It has been proposed to call the standard of light a "dumas." Well, there could not be a better way to immortalise a great man; but there is no doubt that the system of giving names of individuals to units has been carried a little to excess.

Well, gentlemen, I have pointed out to you how this Congress has decided upon a system of units of electrical measurement; how we have agreed and recommended that administrations should collect together observations upon lightning, thunderstorms, lightning-protectors, and earth currents; and how we have agreed upon an unsatisfactory standard of light. But we have done this: we have proved by these Congresses in Paris, that at any rate there are some questions that can be amicably

and pacifically solved by international meetings. When we look back upon those meetings, those who were present, there are two or three facts that come back forcibly to the memory. It was perfectly impossible for any one to have been present at those three Congresses in Paris without admiring and feeling a kind of affection for that man, Sir William Thomson, who was the life and soul of the whole business. Also, it is quite impossible to remember what took place in Paris without remembering the lively presence of the indefatigable Mascart. Nobody who went to Paris can possibly have come back without feeling that in M. Mascart he has left a friend behind him. In the first two Conferences the strong feature was the way the *suaviter in modo*, in which Dumas settled all questions, kept everything smooth, and he has left his name behind him in connection with those meetings. But the central figure, the man whose energy and power of dealing with organisation,—I wish we had him in our House of Commons,—was M. Cochery. Nobody except those who were there can conceive the way in which M. Cochery sat at his table, looked around him, and controlled us all just like a marshal governing his forces. But there is one thing about these Congresses that we shall never forget, and that is the friendships that have been made. The good done is evidenced chiefly by the way in which knowledge has been diffused; but the transaction of ordinary business, especially the business between different administrations, is smoothed and simplified by this feeling of friendship that has sprung up. More than that, the progress of electrical science has been assisted by instilling enquiry, and by stirring up a spirit of emulation to preserve and keep one's country and one's service in the front rank of progress: in fact, there is no doubt that these Congresses of Paris mark

"Footprints on the sands of time."

The PRESIDENT: In inviting discussion on this very important subject which Mr. Preece has so fully brought before us this evening, there are one or two remarks which I should like to make. Mr. Preece commented upon the delay which had taken place in settling these electrical units, but he did not point out

the advantages that had accrued from the delay in deciding the ohm—advantages which come out very strongly in connection with the decision arrived at by the Paris Congress of 1884. At the Paris Congress of 1882 there were only 8 determinations of the ohm which had been previously made which the Committee could consider. There were only 8 determinations existing then from which to judge what the value of the ohm should be. But since that Congress there have been no less than 14 distinct determinations made, bringing the total number of determinations extant up to 22; and of those 22, 19 came very close together indeed, so much so that not one of them differs as much as 1 per cent. from the mean value. I think the advantage of these additional determinations is very great, and settles the question so completely that the delay is in fact a very great benefit.

In addition to the five different methods of which Mr. Preece has spoken, which have been employed for determining the value of the ohm, there was another method arising out of Dr. Joule's determinations, in 1866, of "the dynamical equivalent of heat from the thermal effects of electric currents," which gives the result that the length of the column of mercury representing the ohm is 106·23 centimètres. This determination agrees remarkably well with two of the three determinations made within the past year by Lord Rayleigh, the numbers being 106·27 and 106·24 by Lord Rayleigh, and 106·23 by Joule; and it is almost exactly the mean of the two latest values obtained during the present year by M. Mascart and by Dr. Wiedemann. This points to the very great accuracy arrived at by Dr. Joule in his determinations 20 years ago. Of the 4 determinations made at the University of Cambridge under Lord Rayleigh, the values obtained in the three later experiments by three distinct methods show a remarkable agreement, and the second value by the Weber method is regarded as more trustworthy than the first. Besides the 22 determinations of the ohm, there have been four different determinations of the relation between the Siemens unit and the British Association unit. The first determination of Lord Rayleigh gave ·95365 for this ratio; but the mean of his later results is ·95412, whilst M. Mascart's determination is ·95375.

The value ·95384 is the mean of all these determinations, but probably greater weight should be given to Lord Rayleigh's later determinations. The following values in ohms of the Siemens mercury unit and of the British Association unit are obtained by taking the mean of the 19 different determinations which do not differ so much as 1 per cent. from the mean value. The length of the column of mercury 1 square millimètre in section at the temperature of 0° C. which has a resistance of 1 ohm is given in centimètres in the table.

Comparison of different Determinations of the Value of the Ohm.

Date.	Observers.	Siemens unit.	B. A. unit.	Column of mercury.	Methods.
				CM.	
1881	Rayleigh & Schuster...	0·9438*	0·9893	105·96	B. Assoc.
1882	Rayleigh	0·9411*	0·9865	106·27	"
1882	H. Weber	0·9422*	0·9877	106·13	"
1874	Kohlrausch...	0·9442	0·9897*	105·91	Weber (1st method)
1884	Mascart	0·9407*	0·9861	106·31	"
1884	Wiedemann	0·9417	0·9871*	106·19	"
1878	Rowland	0·9455*	0·9911	105·76	Kirchoff.
1882	Rayleigh & Glazebrook	0·9409*	0·9863	106·28	"
1884	Mascart	0·9407*	0·9861	106·31	"
1884	F. Weber	0·9490	0·9948*	105·37	"
1884	Roiti	0·9443	0·9898*	105·90	"
1873	Lorenz	0·9337	0·9787*	107·10	Lorenz.
1884	Lorenz	0·9417	0·9871*	106·19	"
1883	Rayleigh	0·9414*	0·9868	106·22	"
1884	Lenz	0·9422	0·9877*	106·13	"
1882	Dorn	0·9482	0·9939*	105·46	Weber (by damping).
1883	Wild	0·9462	0·9918*	105·68	"
1884	H. F. Weber	0·9500	0·9958*	105·26	"
1866	Joule	0·9415*	0·9869	106·21	Joule.
1884	**Mean value**	0·9431	0·9886	106·03	...

* These numbers have been calculated by taking 0·9540 as the ratio of the Siemens mercury unit to the British Association unit.

In accordance with the decision of the Paris Conference of 1884 on electrical units, the length of the column of mercury

1 square millimètre in section, at the temperature of 0° C., which has a resistance of 1 ohm, is in future to be 106 centimètres; so the value of the Siemens mercury unit is fixed at 0·9434 ohms. The ratio adopted by the Committee of the British Association on electrical units for the ratio between the Siemens unit and the B. A. unit is 0·9540, hence the value of the B. A. unit in terms of the ohm is ·9889 ohms, and the value of the ohm in B. A. units is 1·01124 B. A. units at 0° C.

I will not further take up the time this evening, becaue I am sure you are all wishing to hear what Sir William Thomson has to say with regard to this settlement of the question of electrical units, on which he has been engaged during the last twenty years.

Sir WILLIAM THOMSON: Gentlemen,—You may imagine the great satisfaction it is to myself to see now universally adopted a system of measurement which has the qualities of accuracy and definiteness, the details of which have been set forth so distinctly before us by Mr. Preece. It is great satisfaction to me to see such a system now introduced, when I remember well the time, and a few persons now present also remember the time (Sir Charles Bright and Mr. Latimer Clark, and perhaps even Mr. Preece, remember the time), when the only standard for the measurement of resistance was a mile of copper wire of such and such a gauge, or, for those who dealt in smaller gauges, a metre of copper or silver wire of specified gauge. The real work of the British Association Committee, helped forward immensely by the happy suggestion of names made by Mr. Latimer Clark and Sir Charles Bright, was to lead this country to that system of definite measurement which has been adopted with the very greatest advantage since about the year 1863. At that time the first attempted measurement of the resistance in absolute measure of pieces of wire of given standards, was made and published by the British Association Committee, although, as we all know, it turned out that there was an error of 1·1 or 1·3 per cent.—more probably perhaps 1·3 per cent. than 1·1 per cent., but an error of at all events not less than 1·1 per cent.—in the determination. But the accuracy of the work founded upon that determination was not at all influenced by the

error until certain definite thermo-dynamic or other deductions from it were made, in which, as Mr. Preece has remarked, the principle of energy came into account.

The mere comparison of standards with one another was not at all affected by that error, and in this respect the British Association has done singularly good service. Nothing, I think, up to the present time, has exceeded the accuracy of the work of the British Association Committee in the comparison of standards, and in the giving out, for commercial use, of standards founded upon the unit which they took as the nearest that their measurements could give to the realisation of the absolute C.G.S. unit of resistance of 10^9 centimetres per second.

Parallel with this work in England, there was in Germany that of Dr. Werner Siemens and his brother, our lost friend Sir William Siemens, who introduced great accuracy in the construction and in the multiplication of standards of resistance, founded upon the well-known Siemens mercury unit. In one respect the system adopted by the Siemens' has been paramount in its importance, and that is, it is a system in which, by a mere definition of quantity and shape of a material obtainable of definite quality, a standard is defined. Now the British Association Committee's standard could not be so defined: it could only be defined as that which agreed with a certain individual primary standard adopted by the British Association Committee. It was only by an interchange of actual pieces of metal, carried about from place to place, that the British Association system could be propagated from its origin; whereas the Siemens system could be reproduced at any place without the transport of standards at all. This was felt to be so great an advantage that, on the primary proposition of Dr. Werner Siemens and his brother, with the cordial support given by others who saw the importance of the proposition, that proposition was adopted as the foundation for the definition of the legal ohm at the 1881 meeting of the Conference. This being once adopted, we have the means of stating in three or four figures what the result of the observations of any observer in any part of the world is in respect of the measurement of the legal ohm. Without this system some-

thing more was required, and that was the transfer of a standard compared with the British Association Committee's B.A. unit. Both systems have been carried out, but results may be translated into, where not primarily expressed by the observer in terms of, the Siemens unit.

As for the choice of the number 106 instead of 106·2, I certainly should have been more pleased if the Conference had adopted with unanimity, or a sufficient approach to unanimity, 106·2. I believe that 106·2 is more probable than 106·1, and more probable than 106·3, although at the beginning of the Conference I may say that it was not considered certain whether 106·2 or 106·3 was the more probable. There were very important observations that led to even a smaller figure than 106, and it was impossible for the Conference to go into the full criticism of the merits of any of these experimental methods. Therefore, without declaring that they would not give weight to the experiments whose results were below 106, it was impossible to unite upon any fourth figure. There was this satisfactory foundation for the choice of the three figures, that we were quite certain that they were all right; and although it might be said that most probably the fourth figure in 106·2 was right, we agreed unanimously that there was no possibility of doubt that the three figures were right in 106. It may be, and I believe it is, throwing away accuracy for any ultimate scientific applications, and particularly for very fine calculations in which the principle of energy comes into account, to take anything less probable than the most probable of the determinations, and that seems to me to be those of Lord Rayleigh, Mr. Glazebrook, and M. Wiedemann, which come all to the 106·2. I think that in a scientific sense we are throwing away accuracy to neglect the probability of the ·2 being more near right than ·0. Still, I quite see that the Conference was right in its conclusion, and I cheerfully acquiesced in the decision, and in fact I gave up the proposal of 106·2 in order to adopt the three figures 106, about which the opinions were absolutely unanimous.

There remains the very great satisfaction that if, as no doubt will be the case when a year or two have passed, we have

a certainty as to the fourth figure, a certainty that it is 1, 2, or 3, all that will be necessary in respect to any box of resistance coils made in accordance with the legal ohm of 1884, to bring it into agreement with the more accurate scientific determination that we may expect, will be to alter the label of the box on which the temperature at which it was correct is stated. I believe it is quite to be expected that within the next two or three years we shall know for certain what is the fourth place: as to whether we shall know the fifth place I will not say. I believe it to be most probable that we shall find the four figures to be 106·2; and to correct a box of resistance coils of German silver which had been made correct to the legal ohm at, for example, the temperature of 10° C., we shall only have to substitute, instead of 10° C., 16° C. as the temperature at which it will be correct to the scientific ohm of the future determination, which no doubt would soon come to be the new legal ohm. (I see some one doubts this six degrees. Let us calculate it. Suppose the 106·2 to be right, a correction of one-fifth per cent. is required: one-thirtieth per cent. is the change of resistance of German silver for one degree, and six times one-thirtieth per cent. is one-fifth per cent.; hence six degrees centigrade is the correction that would have to be made on the supposed temperature for which the box of resistance coils are right.)

The change from the legal ohm of 1884 to any future legal ohm, or any future legalised result of more accurate scientific investigation, will thus be absolutely without inconvenience in the practical form of the value of the resistance coils which have been made according to the legal ohm of 1884. I think, therefore, no one can say that the Conference did not judge wisely in taking 106 as it did.

I am afraid I cannot quite concur with my friend Mr. Preece in what he has said of one of the conclusions of the Conference, that referring to the choice of a unit for light. I think the Conference was right in its conclusion. I myself, when the subject was put before us a year and a half ago, did not then think it probable that such a conclusion could be accepted, but I have since felt compelled, by the evidence laid before the Conference, to admit

that M. Violle's work had given a foundation for a definite choice of a photometric unit; and I was glad to agree with the proposal made by M. Jamin, and amended by Professor Helmholtz, which defines quantities of light in the terms we have heard just now from Mr. Preece.

There are two or three interesting and curious points connected with standards for photometry to which I should like to call attention, if I do not detain you too long. In the first place, we may pass away quickly from candles and gas flames, and from the very beautiful lamp placed before the Conference by Dr. Werner Siemens, in which the wick is arranged to give a flame of definite length, and with a definite chemical substance burning in it; because, though important and valuable as secondary practical standards, not one of them gives an intrinsic standard for the reckoning of light.

The only intrinsic standard before the Conference was that proposed by M. Violle, and that itself was a reason for adopting it; unless it could be proved that it was not an intrinsic standard, or that it was not practically capable of accurate reproduction. M. Violle's experiments prove that it is capable of accuracy—capable, probably, of as great accuracy as photometric methods can use. I do not say that I consider it absolutely certain that there is that degree of accuracy, but I think that M. Violle's experiments do give very strong evidence in favour of the assertion, that the light given out by incandescent platinum at the solidifying point is producible practically, with conditions that do give it definiteness, to as great a degree of accuracy as photometric measurements can use. Well, if this unit can be produced with reasonable facility, and can be used for the accurate standardising of other standards, and if it was the only intrinsic standard before the Conference, surely there is everything to be said for their choosing it. On the other hand, we know that it is exceedingly difficult to produce. Still, what M. Violle has done surely others can do. It is very expensive to produce, and that is a reason which might be fatal to it if we depended upon our Government for assistance. I asked M. Violle himself, and he said that it would cost about 6,000 francs, or £240. It requires two

kilogrammes of platinum. The subject of the requisite purity of the platinum was discussed at considerable length, and it was stated that it could be obtained as a commercial product from Messrs. Mathey & Johnson, in a degree of purity sufficient for obtaining the results desired in M. Violle's arrangement. Besides the two kilogrammes of pure platinum, necessarily very costly, there is the preparation of a suitable gas furnace, which is troublesome, but not impracticable, and altogether it seems to become a question of £240.

Well, now, let us put that £240 against the cost of making an incandescent light. I doubt very much whether any one, beginning from the beginning, could produce an incandescent lamp, which could constitute any approach to a photometric standard, for £240. I think in point of cost alone, M. Violle's method would be chosen rather than the method of incandescent lamps; but at the same time I cordially sympathise with Mr. Preece's remark about incandescent lamps, and about the importance of looking to them in connection with the foundation of a unit for the measurement of light. I think we are within measurable distance of finding a unit of light in the incandescent lamps; but I deny the practicability, in the present stage of practical science, of measuring the surface of carbon filaments of ordinary incandescent lamps with any very minute degree of accuracy, or of fixing precisely the temperature of the incandescence of the filaments. It must be observed that to get a definite basis for photometry in an incandescent lamp, it would be necessary to know precisely the temperature. I do not mean necessarily in degrees of the air thermometer, or of the absolute scale, but to have the means of securing that the temperature of the surface of the carbon filament shall be a certain and definite temperature. I believe that with the same temperature of the surface, the incandescence would be the same with all carbon filaments, whether the Edison, the Swan, or the Lane-Fox, or any other, or with carbon in any shape. It seems probable, though I am not certain, that there are no two kinds of carbon so unequal with respect to the degree of incandescence that they give at the same temperature; but then how is the

temperature to be fixed? There is one way in which it might be supposed that we could define the incandescence, supposing the area of the radiant surface to be known, and that is by measuring the number of watts of radiant activity per square centimetre of surface. But all that in the present state of practical science we can tell is, the number of watts actually spent in the generation of heat in the filament: how much of this heat is carried off per square centimetre of surface by convection of the residual air in the lamp, and how much is radiated, we cannot tell. It is certainly very different in different lamps, because of the difference of perfectness of the vacuum. With two lamps having the same magnitude of carbon filament, the same resistance of carbon filament, and at the same temperature, the light is brighter in one than in another, on account of one containing less residual air than the other. The temperature produced by a certain degree of activity in the generation of heat is lower in the lamp in which there is the greater convection, so that, unless we are sure of our vacuum, the number of watts of radiant activity per square centimetre of surface is not a sufficient definition of temperature.

There remains Captain Abney's proposal, and that is the ratio of the quantity of light of a certain definite quality—say, for example, B light—to that of another definite quality—for instance, E light: that would give an absolutely perfect scientific definition of the temperature. If then we could get a perfect measurement, or a perfect enough measurement of the area of the carbon filament, and if we could secure, to the requisite degree of accuracy, the measurements of the ratios of the quantities of two different rays emitted from the surface, then we should have a perfect scientific foundation for photometry, as set forth in Capt. Abney's paper recently communicated to the Royal Society. It seems to me probable, however, that the difference in the whole quantity of light given out from time to time by any source may be very considerable before there is any perceptible, or accurately enough measureable, difference in the ratio of the quantities of the two different rays. Be this as it may, however, we should still be far from a foundation for photometry in any of the

incandescent lamps hitherto made, because of the difficulty of accurately measuring the area of the carbon filament. There was in truth no other foundation for a photometric unit before the Conference than that of M. Violle, and from the skilful and devotedly persevering work which he has applied to the subject, we may confidently hope for valuable practical results.

The PRESIDENT: I will now ask you to allow me to express your hearty vote of thanks to Mr. W. H. Preece for his communication, and also to express our great pleasure at having Sir William Thomson with us this evening, and to thank him for his valuable contribution to the discussion on this important question.

Carried unanimously.

A ballot then took place for Mr. Schwafe, who was elected as an Associate, as were also the following:—

Foreign Members:

Gabriel Verrier. | John Swartz.

Associates:

R. Ernest Fletcher. | Alexander Pelham Trotter.

Samuel Henry Henry.

The meeting then adjourned.

Professor HUGHES communicated the following in reference to the Standard of Light:—

International Congress of Electricians, Paris, September, 1881.

	Standards of Light.	Proposed by	Nationality.
1	Carcel lamp, consuming 40 grammes per hour of purified Colza oil ...	Dumas, Crova, Felix Leblanc, Allard,	France. " " "
2	English standard candle	Col. Webber, Shoolbred,	England. "
3	Magnesium light	Neujean, Flamache,	Belgium. "
4	Drummond light	Neujean,	Belgium.
5	Schwendler's standard—platina wire heated to incandescence by a given electric current	Tchikoleff, Bede,	Russia. Belgium.
6	Violle's standard, 1 centimètre square of melted platina at the point of solidification	Violle,	France.
7	Violle's standard of melted silver at its point of solidification	Cornu,	France.
8	Incandescent irridium wire of a certain length, through which passes the unit of current	Sir W. Siemens,	England.

International Congress of Electricians, Paris, October, 1882.

1	Violle's standard of melted platina	Dumas, Violle, Broch, Helmholtz,	France. " Sweden. Germany.
2	Schwendler's standard of incandescent platina wire, through which passes the unit current	Wiedermann,	Germany.
3	Vernon Harcourt Pentane standard	Dr. Werner Siemens,	Germany.

International Congress of Electricians, Paris, May, 1884.

1	Violle's standard of melted platina, 1 centimètre square at its point of solidification	A series of experiments on this standard were reported to the Congress, and it was finally adopted unanimously.

A CONFERENCE, under the auspices of the Telegraph-Engineers and Electricians, on "Electric Lighting in Relation to Health," was held on Friday, July 4th, 1884, at the International Health Exhibition—Professor W. GRYLLS ADAMS, F.R.S., in the Chair.

The CHAIRMAN, in opening the proceedings, said the Society of Telegraph-Engineers had come forward, in consequence of an invitation from the Executive Council of the Exhibition, to conduct this Conference, which it was their desire to make bear upon the main subject of the Exhibition. In the columns of a leading newspaper there had been some remarks as to the difficulties which must sometimes be found in making out the connection between the subject of some of these conferences and the laws of health; but, with regard to this particular subject, there would be no difficulty whatever in showing that there was a close connection between the subject of electric lighting and the health of the public generally, whether it were in the large, crowded workshops of the country or the homes of the people, where many have wished to have the electric light introduced; though their hopes in that direction might perhaps have been a little greater a year and a half or two years ago than they were at present, still there was reason to believe that those hopes would soon be revived again.

ARTIFICIAL LIGHTING IN RELATION TO HEALTH.

By R. E. B. CROMPTON, Member.

In early times but a small fraction of our forefathers' lives was spent under artificial light. They rose with the sun and lay down to rest shortly after sunset. During the long winter evenings they sat round the fire telling stories and singing songs of love and war; the fire-light was sufficient for them, except occasionally during grand feasts and carousals, when their halls were lighted by pine wood torches or blazing cressets. But, as a rule, after sunset they lived in semi-darkness.

From that early period, as man has advanced in civilisation, in the thirst for knowledge derived from books, and in following the

gentler pursuits which demand an indoor life, there has been a steady increase in that fraction of our lives which is spent under light other than that of the sun. But the improvement in the quality of the artificial light has been very slow. The ruddy lights and picturesque shadows so faithfully handed on to us by Rembrandt's pictures show us very graphically what our poets have called "the dim glimmer of the taper" of those days. A few years before the introduction of gas, Argand, by his improvements in the burners of oil lamps, enabled our fathers to see for the first time a comparatively white light, but as far as the matter we to-day propose to discuss is concerned, viz., the effect of artificial lighting, and more particularly electric lighting, on our health, we need only consider the reign of artificial light as it commenced with the general use of gas and petroleum, for then and then only could it be said to affect our health.

Prior to the introduction of the electric light we have been accustomed to consider hours spent under artificial light as hours during which all conditions are less favourable to perfect health than they would be during daylight. Can we now hope to ameliorate this condition of things through the agency of electricity? Before we can discuss this question I must point out to you the chief differences which exist between hours of work or recreation spent in daylight and under artificial light. In the former case we live in abundance of light. The sunlight itself exercises a subtle influence on our bodies; that mixture of heating and chemical rays which when analysed form the solar spectrum, and combined form the pure white light of daylight, is needed to enable all animal and vegetable organisms to flourish in the fullest conditions of healthful life.

In nearly all cases when the sun is up the functions of life are in the state of fullest activity, and when it sets they sink into comparative repose. In daylight life wakes; in darkness life sleeps. In addition to the abundance of pure white light, the heat attending sunlight is only that necessary for health. The air remains unvitiated, except by our own breathing. On the other hand, when working under artificial light, we have these conditions all altered in degree.

1st. We have an insufficient light; as a scale of lighting by gas or by electricity which would be pronounced excessive at night time is still far inferior to average daylight.

2nd. All artificial lights, whether produced by combustion, as in the case of candles, oil, gas, and petroleum, or by the incandescence of a conductor by the means of electricity, produce heat; this heat, in proportion with the light afforded, is enormously in excess of the heat given by sunlight. Electricity, as you will see hereafter, is far the best in this respect, but even it is inferior to sunlight.

3rd. All these same illuminants, excepting electricity, contaminate the air, and load it with carbonic acid, sulphur, and other compounds—all injurious to the health and to the general comfort of the body.

It will be convenient to consider the effects—first, on our health generally; second, on our eyesight in particular. I have already called your attention to the fact that that proportion of coloured rays which when combined form white sunlight is that best suited to healthy life. It is necessary for that sufficient and proper stimulus to the organic changes which go on in our bodies, and which we call a state of good health. The various artificial lights differ very widely from sunlight in this respect, that they are all more or less deficient in the rays at the violet end of the spectrum, commonly called the actinic rays, and which most probably exercise a very powerful effect on the human body. It is the want of a due portion of these violet rays which makes all artificial light so yellow. Even the light of the electric arc, which is richer in these rays than any other, is still on the yellow side of sunlight. The incandescent electric light is next best in this respect; next in order comes gas, petroleum, and the various oil lamps. No doubt some of you will challenge my statement that the electric arc is yellow. It has always been called a cold blue light. It is not so; it is only by comparison with the yellower light of gas or with the incandescent lamps that it appears blue; when compared with the sunlight reflected from a white cloud it will be seen to be distinctly yellow in tinge, but still both classes of electric light are far superior to all others in nearest approach-

ing the white light of daylight, and thus satisfying the actinic action which our bodies demand.

Turning now to the comparative heating and air-vitiating properties of artificial lights, which we shall find it convenient to take together, I have here a table (Table A) prepared by Dr. Meymott Tidy, which shows the oxygen consumed, the carbonic acid produced, the air vitiated, and the heat produced by the combustion of certain bodies burned so as to give the light of twelve standard candles, to which Mr. R. Hammond has added the heat produced by a 12-candle incandescent electric lamp.

TABLE A.

Showing the oxygen consumed, the carbonic acid produced, and the air vitiated, by the combustion of certain bodies burnt so as to give the light of 12 standard sperm candles, each candle burning at the rate of 120 grains per hour.

Burnt to give light of 12 candles, equal to 120 grains per hour.	Cubic feet of oxygen consumed.	Cubic feet of air consumed.	Cubic feet of carbonic acid produced.	Cubic feet of air vitiated.	Heat produced in lbs. of water raised 10° F.
Cannel Gas	3·30	16·50	2·01	217·50	195·0
Common Gas	5·45	17·25	3·21	348·25	278·6
Sperm Oil	4·75	23·75	3·33	356·75	233·5
Benzole	4·46	22·30	3·54	376·30	232·6
Paraffin	6·81	34·05	4·50	484·05	361·9
Camphine	6·65	33·25	4·77	510·25	325·1
Sperm Candles... ...	7·57	37·85	5·77	614·85	351·7
Wax	8·41	42·05	5·90	632·25	383·1
Stearic	8·82	44·10	6·25	669·10	374·7
Tallow	12·00	60·00	8·73	933·00	505·4
Electric Light	none	none	none	none	13·8

From these figures you will see that the air of a room lighted by gas is heated twenty times as much as if it were lighted to an equal extent by incandescent electric lamps. When arc lamps are used the comparison is still more in favour of electricity. You will be surprised to see from the table that our old friend the tallow candle, and even the wax candle, is far worse than gas in the proportion of air vitiated and heat produced, and you will he disposed to disbelieve it; but the fact is that so long as candles were used light was so expensive that we were obliged to be content with

little of it—in fact we lived in a state of semi-darkness, and in this way we evaded the trouble. It is only since the general introduction of gas and petroleum that we have found what an evil it is.

It is not unusual, in fact we almost always find the upper stratum of air of the rooms in which we live heated to 120 degrees after the gas has been lighted for a few hours. We have grown accustomed to this state of things, and are not surprised that when we take the library ladder to get a book from the upper shelf we find our head and shoulders plunged in a temperature like that of a furnace, producing giddiness and general malaise. If you look again at the table you will see that each gas burner that we use consumes more oxygen and gives off more carbonic acid, and otherwise unfits more air for breathing, than one human being, and it is this excessive heating and air vitiation combined which are the main causes of the injury to the health from working long hours in artificial light. I could go on for a long time giving instances of the fearful state of the atmosphere of our large public buildings as well as of our private homes after the gas has been lighted for a few hours, but this paper is not intended as an onslaught on gas; moreover, these ills are so well known to nearly all of you that I need not bring them more prominently before you. I will only take one instance, viz., that of the Birmingham Town Hall, which has been lighted alternately by gas and electricity.

During the grand Birmingham Musical Festival which was held in that hall two years ago, some careful experiments were made to show how the orchestra and audience in the hall were affected by the two kinds of lighting. The gas lighting was in the form of several huge pendants suspended down the centre of the hall. The electric lighting was in the form of clusters of lights placed on large brackets projecting from the side walls, with two central pendants spaced between the gas pendants. The candle-power given by the electric light was about 50 per cent. in excess of that given by the gas light, the degree of illumination by electricity was consequently very brilliant.

It was found that when the gas was used the temperature near the ceiling rose from 60 degrees to 100 degrees after three hours'

lighting. The heating effect of the gas was therefore the same as if 4,230 persons had been added to the full audience and orchestra of 3,100. Similarly the vitiation of the air by carbonic acid was equal to that given off by the breathing of 3,600 additional persons added to the above audience of 3,100. But on evenings when the electric light was used the temperature only rose one and a half degrees during a seven hours' trial, and the air, of course, was only vitiated by the breathing of the audience. The further experiment was tried of giving to every member composing the large orchestra a printed paper of questions asking him how the new mode of lighting affected him or her personally, and I have here 265 replies to those questions. They are very interesting. I will read a very few of them out to you.* From

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* *The following are some of the answers to Mr. Crompton's questions to the band and chorus :—*

"Well lighted, the atmosphere of the room greatly improved, and the equal temperature of the whole building has greatly diminished the risk of taking cold from the draughts in entrance and exit."

"Much more pleasant than on any other occasion, in fact I never saw a more brilliant light in any hall or concert-room in my life. It is a great success, I should imagine."

"Well lighted, very much cooler, thus allowing *us* to warm to our work by our own exertions, and also to cool down by resting. A general satisfaction is expressed at the improvement of the comforts of the workers."

"At a comfortable, even temperature, the light has not afflicted me in the least, although I sit in between four dozen, and on a line to the side gallery lights. The gas used to make me dim."

"I, as a member of the Festival Choral Society, think your improved light is a source of great pleasure to those of us that have to occupy the orchestra so often during the concert season."

"I have never been on the orchestra before this Festival, but I found the part of the orchestra I occupied was as cool as the floor of any previous concert that I have attended in this or the Wolverhampton Hall."

"Cooler and fresher, likewise the violin strings of my own instrument have not been subjected to variation in pitch or otherwise, as is often the case with gas, and think this is important on such occasions when a cool atmosphere is an acquisition to a body of instrumentalists."

"Lower in temperature, could see music books better. Freedom from oppressiveness—attended six previous Festivals, some one has fainted. I believe this has not happened this time."

"Very cool as regards temperature compared with the temperature when

them you learn that without exception the comfort and general well-being of this large orchestra was increased enormously by the use of the new illuminant, and it is reasonable to suppose that the comfort of the audience was increased in an equal degree. I must here add that I have to thank Mr. Henry Lea, of Birmingham, who conducted these experiments, for kindly allowing me the use of his papers. Now, we all of us know that the times

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gas has been used to light the orchestra. Instruments have been better in tune with organ. Light excellent."

"Very much cooler than on former occasions, and free from that nasty gaseous vapour that made breathing difficult, and parched the throat and injured the voice."

"Very much cooler than is usually the case with gas. The light is very pleasant, being a cool white light, not trying to the eyes, always keeping the same colour, never jumping, as I have found the case with other electric lights."

"Exceedingly cool and agreeable. Certainly one of the most pleasing reminiscences of the 1882 Festival will be the advantages of your electric light."

"Exceedingly comfortable, the light brilliant, at the same time of a character which does not cause any strain upon the sight. I mention this as I have weak eyes."

"Delightfully cool, and at the close of the performance but very slightly different in temperature than at the beginning. It is vastly superior to the old system of lighting by gas."

"Brilliantly and equally lighted; whilst from my elevated position on the uppermost seat by the side of the organ, had the hall been lit by gas the heat would have been simply unbearable."

"Very much cooler than on other occasions, even in the depth of winter; and situated, as I have been, within one seat of the top, where the atmosphere is usually unbearable, I have felt extremely grateful at the comparative coolness of temperature."

"Deliciously cool and comfortable, no comparison with any preceding Festival, the heat at the end of the evening being scarcely greater than at the beginning. There has not been one case of fainting near me."

"Remarkably cool; in fact the change from the shifting atmosphere under ordinary circumstances is wonderful. I sincerely hope that arrangements can be made to secure this lighting as a permanency."

"Much more comfortable in consequence of the substitution of the electric light for gas; it has been but very little warmer for the evening performances with the light than for the morning performance. I have sung in all the principal halls in London (including Royal Albert Hall, Kensington), and have always found the orchestra quite unbearable soon after lighting the gas. I have pleasure in speaking most favourably of your light, and the comforts in every respect attending it."

when we suffer most from the effect of artificial light is in crowded places of public amusement, which are at the same time brilliantly lighted. Many of us are unable to go to the theatre or to attend evening performances of any kind, as the intense headache which invariably attends or follows our stay in such places entirely prevents them. This headache we commonly say is inseparable from the heat and glare of the gas. Now this phrase is not strictly correct. It is no doubt due to the heat of the gas and its air-vitiating properties, but when we use the word glare I believe we refer to the effect the gas light has upon our heads, and which effect is not due to excess of light. On the contrary, I believe if a far greater amount of light be given by the electric light without the heating and air vitiation being present such headache is never produced, although some of the more tender-headed amongst us will at first complain of the glare because they are habituated to associate plenty of light with great heat, great air vitiation, and other evils.

Indeed, so long have we been accustomed to closely associate brilliant artificial light with headache and glare, that we who are introducing electric light are most cautious not to give the full quantity of light which we could afford to give, and which would afford the greatest rest to the eye and greatest bodily comfort. I now come to the effect that light has upon the temperament. If we try the experiment in an assemblage of people of gently decreasing the lighting of the room, it will be found that the spirits of every one will be depressed just as the light is depressed, and, *vice versâ*, their spirits will be raised just as the light is raised. I have many times, when conducting experiments of electric lighting on a large scale, noticed this fact, and I have been led to the conclusion *that during hours of waking every person is benefited by increase of light up to the extent of full sunlight*, providing that this high degree of lighting is not attended by heat and by air vitiation; and I must add that the source of light must not be from one or two brilliant points only, but it must be well distributed and not such as to cause dark deep shadows.

This leads me on to the subject of the effects on the eyesight of the electric light as compared with other lights. *Healthy eye-*

sight demands a plentiful supply of light. It is the greatest mistake to suppose that a state of semi-darkness is good for our eyes, unless they are defective, or recovering from the effects of past injury or disease. Whoever saw a painter, engraver, printer, watchmaker, or indeed any one the quality of whose work depends on the excellence of his eyesight, who did not desire a flood of pure white light thrown on to his work. I think I have the authority of oculists when I say that 19-20ths of the diseases of the eyes arise from working the eyesight long hours with insufficient light. Again, another great cause of injury to eyesight is the unsteadiness of most artificial lights. Much improvement has been made in the light of gas during the last few years by the introduction of argand burners, and globes for the flat gas burners having much larger lower openings, so that the dancing and flickering batswing burner of five years ago is not now common in a good house. But even the steadiest of the modern gas burners is extremely unsteady as compared with the light of the incandescent electric lamp. Those of you who have been to the Savoy Theatre will have noticed the effects of the lights behind the scenes on the scenery itself. The light is so absolutely steady that it is comparable to sunlight. Hitherto I have said nothing as to the comparative excellence of the two forms of electric light, viz., the electric arc and the incandescent lamp. Both have their proper places. The arc light, which is the whitest in colour and most economical to produce, is not so steady as the incandescent lamp. It is therefore unsuitable for indoor use or for reading by, or for such occupations as require the maximum of steadiness. But it is well suited for the lighting of large buildings and public places. I am unaware if any experiments have been made as to the effects of brilliant arc lighting on the eyesight of men who have to work night shifts, as although the opinion of the workmen who have to work under it is unanimous in its favour, yet that opinion is more based on their personal convenience, due to their being able to carry on their work with facility almost equal to that given by daylight than with special regard to their health. The large sorting-rooms at the General Post Office at Glasgow have been for a long time lighted by the

arc light, and with a most beneficial result to the health and eyesight of the letter sorters and telegraph clerks. The former occupation is one which tries the eyesight very severely. The public generally does not know how the habit of writing the addresses on envelopes with pale ink and blotting it off rapidly before it has time to darken tries the eyesight of the Post Office letter sorters. So long as gas is used a powerful burner has to be brought very close to the head of the sorter, and under such conditions the eyesight fails at an early age. At Glasgow Post Office I am able to boast that by the introduction of the electric light I enabled many of the more aged sorters who were commencing to use spectacles to do without them—and even I put back the clock of time, in enabling several who had used them for some years to discontinue them. I am aware that it has been alleged by the opponents of the electric light, whether interested or otherwise, that in many cases the intensity of the light has injured eyesight. I do not think any such cases can be substantiated. Many of us who are in the habit of experimenting with powerful arc lamps have had our eyelids temporarily affected by incautious exposure at too short a distance. Again, over and over I met with the complaint that if I stare at an arc lamp for a long time it will make my eyes ache; the obvious retort being, why should you stare at the light? If you do the same with the sun you will be equally inconvenienced. Before such an audience as this, which is of course familiar with the beautiful electric lighting in the Health Exhibition itself, it is useless for me to enlarge on the many conditions of the electric light as it indirectly affects health. I may only name the many additional pleasures of the eye we get from its use. Our flowers in our rooms do not fade away, and are seen in their true colours. Our pictures or all coloured objects are seen to better advantage. I may mention one thing which would not generally occur to you, that in London certainly an electric-lighted house can be cleaned properly in winter. You may smile at this, but I assure you that the advantage of being able to turn a flood of light into your drawing-rooms and dining-rooms at six o'clock on a winter's morning, and this without taking away the freshness of the air, so that the whole of the cleaning

can be finished, as thoroughly as if done by daylight, before the family comes down to breakfast, is one that must be experienced before it can be thoroughly appreciated. Again, the advantage to the health of our children is simply inestimable. No night lights, no matches need be left about; no gas turned down low is required. A child six years old can be trusted to press a button and so turn the light off or on; the lamps being high and out of reach are not easily broken or overturned; and the air of the children's nursery, even if the light be kept burning the night through, remains pure throughout. Another indirect advantage due to the absence of heat is that it is comparatively easy to thoroughly ventilate and cool during the hot weather a room lighted by the electric light. The heat of gas placed high in the room causes such intense draughts when the windows are open that the discomforts and dangers of the draughts are almost worse than the discomfiture from the heat and vitiated air, whereas in an electric-lighted room there is no difficulty in opening wide all the windows, the draughts produced being so gentle as to hardly be felt.

The CHAIRMAN said it was very important in discussions at any such Conference that both sides of the question should be put forward, and therefore, if there were any in the audience who might be called opponents of electric lighting, or who would rather perhaps be spoken of as the representatives of the present state of things, he hoped they would take part in the discussion.

Mr. ALEX. J. S. ADAMS thought that although the advantages of electric lighting in the abstract had been ably treated by Mr. Crompton, a very important point appeared to have been disregarded. It had become a truism that electricity was the artificial light especially provided by nature, and in comparison with which other means were makeshifts; nevertheless, in his opinion, it was not sufficient upon an occasion like the present Conference, held under the auspices of the Executive of the International Health Exhibition, merely to advertise that well-known fact to the public. A discussion such as this ought to embrace wider ground, and take a more extended view.

Amongst the ills which afflict modern civilisation, not the least

were the effects of sewer gas and burnt coal gas; and yet, whilst much had been said and done in connection with the former, literally nothing had been done to counteract the effect of the latter, although the majority of our houses become during a part of every twenty-four hours containers of contaminated atmospheres; and hence, he thought, the point to be discussed was not so much the superiority of the electric light, but the whole question of artificial lighting, together with the steps best calculated to bring about the general adoption of electricity. To his mind the question of improved artificial light was one of national importance, and freight with difficulties, because, although the electric light wires might be led to people's very doors, no general adoption of the system could be looked for until tenants were assured of a fair return for their outlay, instead of being, as at present, dependent upon the pleasure or avarice of their landlords. It seemed to him not a little remarkable, considering the importance of the subject, that whilst nearly every necessary of life was under the paternal supervision of the Legislature, artificial light was the almost sole exception, save as regards the quality of the coal gas supplied.

No house would be considered habitable that had not an efficient water supply—and the law did not consider it sufficient that good water companies existed, the house *must* be supplied—and a ventilated drainage system. It was not sufficient that a main drain existed, the law compelled its application. And he would ask, Why draw the line at artificial light? why permit the continuance of lung and blood poisoning upon the present alarming scale? Formerly, in the case of sewage and of water supply, the masses of the people were content with what accidental circumstances provided, *i.e.*, the cheapest or none, until the State stepped in to protect folks from themselves and from their landlords; the same necessity for legislation in respect to the provision of artificial light exists now. Let the actual owners of property be held responsible for the due freedom of their properties from atmospheric poisoning in connection with gas lighting, and electricity, he thought, would come in by natural selection.

Mr. JAMES N. SHOOLBRED remarked, that among the many advantages which the International Health Exhibition presented

for the benefit of the community at large, in none, probably, was the improvement so marked as in that which resulted in the atmospheric conditions of dwellings by the use of the electric light as an illuminant, in lieu of gas, oil, or candles.

The best thanks of the Society were therefore due to Mr. Crompton for the clear manner in which, in his able communication, he had pointed out some of these advantages.

There were one or two additional points, however, which he himself might allude to, as being of interest; and as having come within his own observation.

In the first place, with respect to the products arising from illumination by the electric arc. In the year 1879, the late Mr. F. J. Evans, assisted by Mr. A. C. McMinn, both of the Gas Light and Coke Company, carried out a series of experiments, at which he himself had assisted.

Almost at the same time Professor Dewar, F.R.S., had carried out a very similar series of experiments at the Royal Institution, the results of which were communicated to the Royal Society.

In both cases the experiments were made upon the hourly products of a small-sized Siemens lamp, carefully enclosed, for the nonce, in an air-tight box, duly provided with an air supply and an exhaust pipe. The result of careful analyses of the hourly products gave, in Professor Dewar's case an average of 7·6 grains of nitric peroxide, and in Mr. Evans', an average of 5 grains of nitrous acid. While a corresponding illumination (1,000 candles) by coal gas would have produced about 80 grains of sulphurous acid.

The hourly amount of carbonic acid given off by the electric arc was also found to amount to about ¼ cube foot, or one half of that given off during the same time by the respiration of an adult.

Mr. Crompton had alluded to the improved temperature of the Birmingham Town Hall during the last Musical Festival, by the use of the incandescent electric light; more especially in the upper parts of the orchestra.

The same remark applied to the last Musical Festival at Norwich, where the St. Andrew's Hall had been previously lit by Mr. Crompton by means of arc lights. This illumination wa

…rs' …	Library. A or No. 1	Outside (Court).	REMARKS.
…h.	Degs. Fah.	Degs. Fah.	
	63	50	
	64	48	*Data.*
	64	46	1 Electrical h.p. hour = { 746 watt-hours. 2,564 heat units.
	64	45	1 16-c.p. 100-volt Edison lamp hour ... ... } = { 75 watt-hours $\frac{1}{10}$ h.p. hour. 256 heat units "
	63	40	N. B.—Only $\frac{1}{10}$, say, of this heat is actually conveyed through the glass to the surrounding atmosphere.
	63	38	
	63	36	
	64	36	
	64	50	**Retiring Room, East.**
	64	48	Electrical illumination, 9 16-c.p. 100-volt Edison incandescent lamps.
	64	47	Hourly products of same { Watts ... 675 Heat units, 2,304 = 0·9 h.p.
	65	47	
	67	67	
	68	69	**Reporters' Room.**
	67	67	Electrical illumination, 20 16-c.p. Edison lamps.
	67	63	Hourly products of same { Watts ... 1,500 Heat units, 5,120 = 2 h.p.
	68	72	
	68	72	
	68	68	**Library, No. 1.**
	68	63	Electrical illumination, 2 14-light electroliers = 28 Edison lamps.
	69	72	Hourly products of same { Watts ... 2,100 Heat units, 7,168 = 3 h.p. nearly.
	70	71	
	68	61	
	68	61	**Large Dining Room.**
	72	77	Electrical illumination, 3 14-light electroliers = 42 Edison lamps.
	72	75	Hourly products of same { Watts ... 3,150 Heat units, 10,752 = 4 h.p.
	72	72	
	72	67	

partly tried at that time, to the satisfaction especially of those in the upper parts of the orchestra, who had previously been half-roasted by the products of the gas illumination.

There are at present but few places, so far, where a direct comparison can be made of the results of illumination by gas and by electricity. Within the precincts of the House of Commons, and the retiring and other rooms connected with it, certain parts, previously lit by gas, have been illuminated regularly for the past twelve months by electric incandescent lights. A series of temperature hourly observations have been taken regularly for many years back, at many points in and around the House. These in themselves form an interesting means of comparison, as regards temperature under the two systems of illumination. But the improved condition of the atmosphere, due to the electric light, and shown thereby, must not be considered as representing by any means the total of the amelioration. For it must be borne in mind —that here a very considerable and efficient system of artificial ventilation also prevails, varying in amount in different parts of the buildings; in the House itself the entire air being changed four times during the hour, while in some rooms the products are conveyed away by special exhaust shafts.

By the kindness of Mr. W. J. Prim, the resident engineer of the Houses of Parliament, a number of temperature observations, taken in the same parts of the surroundings of the House of Commons, both during their illumination by gas as also by electricity, as also at different times of the year, are presented in the accompanying table.

To increase somewhat the interest of the table, Mr. Shoolbred himself had added, he said, under the head of "remarks," certain further information as to the illumination, and the hourly products thereof, of the several rooms selected, under both systems of lighting. These observations might possibly present a more complete comparison of the results, unaided by the artificial ventilation already referred to.

In addition to the above facts, it may be interesting to mention the testimony of the refreshment contractor as to the very marked effect of the electric illumination upon the more

delicate of the viands, especially upon the lobsters. These, even in a room of the unusual dimensions of the large dining-room, became black and unfit for food after a lapse of three hours, under the influence of the gas products; while with the electric illumination these viands remained good for a very much longer period.

It is satisfactory to learn, owing to the very marked improvement and increased comfort in these rooms where the illumination by electricity is in regular use, that the extension of this illuminant to the entire of the buildings of the Houses of Parliament is in contemplation.

Judging, moreover, from the great success which has followed the extensive applications of lighting by electricity, both for external and for domestic purposes, which the International Health Exhibition affords (as did also, in a more limited way, its predecessor the "Fisheries"), as well as from the large number of private individuals and of firms who, despite of the heavy drawback attending the special supply in each case of a generating plant, are providing themselves with this new illuminant, it is to be hoped, that before long some of our municipal authorities, gas owners though some of them may be, will, in deference to the growing demand, themselves become distributors of electricity.

Colonel MALCOLM, R.E., said he would try to pose as one of those who were contented with the present state of things. Mr. Crompton had given a very able paper, bringing together a mass of facts which might perhaps, by labour, have been collected by any one of the audience for himself; but there were very few who would care to take the trouble, and many would not be able to get hold of all the data which Mr. Crompton had been able to collect. It was very clear that if the electric light was as easy to manage as he made out, everybody would clamour for it, and electric lighting companies would be doing a large and lucrative business. Apparently that was very clear to the authorities, for they took as much care as possible that everything should be properly regulated; and the point he wished to ask Mr. Crompton was, whether the electric light was really safe in the house. He knew what gas could do: it was able to blow out windows and things of that kind now and then; but still that, like the dynamite

explosions, generally affected only the kitchen-maids; and he once saw a table set on fire by electricity, although it was in the hands of a very able lecturer. He wished, therefore, to know whether there was practically any danger, because he did not see any mention of this point in the syllabus, and it was of some importance when these great currents of electricity were brought into houses. Mr. Crompton had talked about the glare, and said the sunlight did not produce an unpleasant effect on the eyes, that the only effect was to stimulate the eyesight and do it good, and that the glare complained of with electric lighting and gas lighting was due to the heat, the vitiating products of combustion, and so on. He thought one cause of glare was a bright light proceeding from a small object. If the sun streamed into his bedroom through a small crack, it had a very unpleasant effect. What seemed necessary was to have a number of them well disposed so that if you could only get enough, so that the eye could be attracted to any particular one, he could understand the object of Mr. Crompton's remarks; but he fancied that one bright light, or anything that would be perpetually attracting the eye, would have the effect of glare, and would be very unpleasant.

Mr. George Offor said he had been recently engaged in carrying out an experiment in connection with domestic lighting, at Colchester, where the electric light had been supplied to the public on very similar lines to those of the gas supply. If the opposition to the new illuminant was fairly represented on this occasion by one actual sceptic, and one very doubtful friend of the old system, he thought that electric lighting was not at all likely to suffer. The argument used by the first speaker was, that there would probably be a rise of rent in the event of electric lighting being introduced into houses, which was probably the greatest compliment that could have been paid to the electric lighting interest; for if, by introducing the electric light, rents were to be raised, it proved indubitably that the light must be a very great improvement indeed. But was the same principle to be applied to everything else in the house? Would that gentleman refuse to put up a proper system of cleansing his cisterns, for example, for fear his rent should be raised when the landlord heard that he was getting

purer water? The same principle might be applied to everything, until at last it was reduced to this, that in order to get the cheapest rent they must take care that the house was most injurious to health, and a most improper place for a dwelling. From his experience during the last few months, his impression was that the public were never so eager to have the electric light as they were at the present moment. The question had been asked of him hundreds of times since the opening of the Exhibition, "Why cannot we have the electric light?" and he must quote from memory a letter he had received only a few days ago from an inhabitant of South Kensington. That gentleman wrote to say they were anxious to have the electric light, but could not get it. A Provisional Order had been granted to a company for the purpose of lighting South Kensington, and could not he induce the company to part with it for a consideration, and carry out the Order to give them the benefits of electric light. He had also applications from residents in some of the great squares, saying they could not imagine why they were unable to get the electric light. There were practical difficulties, and it was to get over those practical difficulties that the installation at Colchester had been carried out. Much as people wanted the electric light, they were not prepared to put down the necessary plant in their houses; but if they could have a wire carried into the house like a gas pipe, which gave them electricity in the same simple way that they got their gas, and were charged so much per quarter for the quantity of electricity they used, any amount of business might be done. The practical difficulty was how to do this. In London, where were there houses which had the accommodation for putting up gas or steam engines, and dynamos or storage batteries? Therefore they must be supplied by means of central stations; and this was what has been done at Colchester. He mentioned this because of its interest at the present moment, although it was hardly within the limits of a Conference dealing with the effects of electric lighting on health. But he really thought they were engaged in killing a dead donkey over again when they got up and addressed an intelligent audience on the subject of the superiority of

electricity with regard to health. It was admitted on all hands that it was superior, and the public were beginning to understand that it was not a question of whether electric lighting could be supplied at the equivalent of 3s., 4s., or 5s. per 1,000 feet for gas; for there were many who were intelligent enough to know that if they could save their doctors' bills and upholsterers' bills—if, as a well-known universal caterer told him a short time ago, he could save £500 a year in whitewash—surely it would not be a question as to whether electric lighting would cost him just the same as the same amount of light from gas or other illuminants. The practical question was, could the light be supplied in a convenient way? That had been now done at Colchester. The experiment was a most interesting one, and proved perfectly successful. There they had a central station where the current was generated, and in different parts of the district there were other stations where it was stored, and from those stations it was supplied to the houses by small wires, in the same way as gas from small pipes. If that system could be carried out everywhere, the problem would be solved; but it must not be supposed that there was any lull on the part of the public. The public did make a mistake, no doubt, two years ago, when they knew very little how the electric light was produced; as an illustration of which he might mention a joke which occurred at the Crystal Palace Exhibition, where a couple from the country, having asked the price of an incandescent lamp at one of the stalls, and being supplied with the lamp for 5s., expended a box of matches in trying to light it, and then declared that the whole thing was a swindle. There was no doubt an idea at one time amongst some of the public that the electric light could be supplied in as simple a way as that; but there was no doubt now that they were perfectly prepared to have it, if you could give it to them with the same convenience as gas. The public must not suppose there was any doubt about their having the electric light if they chose, but they must find the money in order that the new illuminant might be provided by those who knew how to do so if the means were placed at their disposal. If the public would not find the money to purchase the plant, it was no use to expect that the light could be supplied. The gas

interest had an enormous amount of capital at their disposal. Let the public thoroughly know that the electric light could be supplied with all its advantages; and if they had faith in their opinions, and would subscribe the funds, there were gentlemen in that room who would undertake that the whole Metropolis was well lighted.

Mr. J. Swan said he could give a very direct answer to the question put by Colonel Malcolm as to the safety of electric light. His own house was lit with incandescent electric lamps, and had been so lit for more than a year. During that time it had been under the control of servants, just as gas lighting ordinarily was. There had never been any accident from fire or from shocks, but they had had a perfect light, without hitch or discomfort of any kind; in fact, they had experienced infinitely more comfort than could possibly have been obtained from either gas or lamps. If Colonel Malcolm would come and see him, he would give him a hearty welcome, and substantiate these statements. The speaker following Mr. Crompton had said that if electric light was of such great importance to health as had been contended, then it was the duty of the Legislature to see that the public were put in possession of the benefits it was capable of conferring. He seemed to have the impression that the whole matter of lighting had been neglected by the Legislature, not only as regards electric light, but also as to gas light. This was not so. When power was granted to a gas company, the Legislature required that the gas should be of a certain quality as to freedom from sulphur and other deleterious agents, and also as to illuminating power. It was also a fact that the Legislature had intervened in the matter of the public supply of electric light; but he was afraid that this intervention had so far not been attended with happy results. With an honest intention on the part of Mr. Chamberlain and the Board of Trade to help forward the introduction of electric light, on the indisputable assumption that it would be a public benefit if it could be adopted generally, an Act of Parliament was introduced to protect, on the one hand, the public from extortion, and any evils which might possibly grow out of a monopoly; and, on the other hand, companies were disposed to supply the public with this better kind of light. But the Act had not been very

successful in the attainment of these objects. It had not sufficiently recognised the fact that electric lighting was an infant and required nursing, and not a dangerous adult in need of a "strait jacket." The Act had in fact *discouraged* electric light companies from attempting the supply of electric light to the general public on a large scale, and was a chief cause why more had not been done in that direction. It would be a good thing if one of the practical results of that Conference was an endeavour to obtain such alterations in the Act as would make it more of a stimulus, or less of a hindrance, to the extension of electric lighting.

It was clearly evident that the public were desirous of having electric light supplied to them. They knew it was of the utmost advantage to cleanliness and health. He had received many communications similar to that mentioned by Mr. Offor, asking why electric light could not be supplied to this and that locality. His reply was, that in the face of an Act of Parliament, which, in the endeavour to protect the public against possible but remote dangers, had so embarrassed a new and undeveloped enterprise by excessively hard and onerous conditions, sufficient inducements did not exist to lead capitalists to invest their money in extensive electric lighting operations of a pioneer character.

It was most desirable that some alteration should be made in the Act; in the public interest, and especially in the interest of the public *health*, an effort should be put forth with that object.

Mr. T. R. Crampton said the whole question of electric lighting had been very fully put forward by Mr. Swan. It had now become mostly a commercial one. Its practicability had been sufficiently solved, at least so much as to justify the public to come into it, if the inducements to do so were sufficiently fair as between buyer and seller. But the Act had peculiar clauses in it. Public companies had perhaps to go through various vicissitudes before they received any interest on their capital; but when they had made a success, a town or a corporation could come forward and forcibly purchase the rights of the company—not on so many years' purchase, or on any terms

which would give the shareholder an equivalent for his investment, but the purchase could be made at the cost of the material on the ground. No progress could be made in the face of such conditions. He might say that, so far as he had watched it, there was no need to go into the question of health—that was solved, and the question of convenience was solved. We all know what a beautiful light it gives; and what was perhaps a greater question than all to the female portion of the population, viz., that of cleanliness, that was also solved. He could give a case where the cost of taking down pictures and furniture to clean every year had been more than the whole cost of the gas supplied during the time. Any one who thought about it would be glad to give double the price of gas for the electric light, and he often asked his friends why they did not give it. The only answer he received was that they could not provide and look after the plant necessary for its installation. Doubtless it took a certain time to introduce anything, however good it might be; and he believed no long period would elapse before companies would be in a position to supply private establishments.

Mr. Greenhill said he knew a gentleman in Scotland who had had the electric light in his house for nearly two and a half years, and he told him a short time ago that if it cost five times as much as it actually had done he would not abandon it. It could not be said that this gentleman sacrificed his pocket for the sake of electric lighting, because he did not think his knowledge extended much beyond the difference between a dynamo and a lamp.

Last week, at Stockton-on-Tees, a gentleman had told him he had had the electric light in his house for six months, and if gas were supplied free he would not abandon the electric light. He believed the public were now more anxious than ever for the electric light; and as to their subscribing money to carry it out, they had subscribed a very large amount, but, unfortunately, in many cases it had not been applied in a manner which had borne satisfactory fruit. The electric light had passed through the same stage as railways had on their introduction, namely, the speculative stage; and he believed that now electricity was on a firmer

basis than ever before, and in a short time he was certain that the electric light would become general.

Mr. A. J. S. ADAMS, in reply to a previous speaker, said that he was a great advocate for the adoption of the electric light, but that the question remained, how was its general adoption to be brought about? It seemed to him that the delegate from Colchester and he were looking at the question from two different points. The former had in his mind's eye people who could and would afford to throw away their gas brackets in favour of electric light fittings; whilst his (Mr. Adams') previous remarks were on behalf of those who would not or could not afford that expense.

Mr. WILLOUGHBY SMITH said that a great deal he had heard to-day was merely a repetition of what he had heard a few years ago. One gentleman had said that the public were clamouring for electric lighting, but that they seemed reluctant to pay for the same. He thought that rather hard on the public, considering that within the last two years they had subscribed no less than twelve millions of money for electric lighting, and have not yet seen any result for such an enormous outlay.

A great deal has been said as to the luxury of the electric light by those who were fortunate enough to possess the same in their own houses, and no doubt it had, as had been stated, many advantages over other systems of lighting; but there are always two sides to a question, and this case was no exception to the rule. He had not the electric light in his own house, but had the misfortune to live in the next house to a gentleman who has. He said "misfortune," because at first not only did each gas light in his and the neighbouring houses diminish in intensity, but also responded to every stroke of the gas-engine, which state of things was only altered by a separate main being laid to supply the engine; but now the click of the engine and the burr of the dynamos was very annoying, especially as he could no longer enjoy, as he used to do, the quietude of his own garden. The engine, he believed, was called a silent one. If it were considered so, he often wondered what a noisy one must be like. Knowing what he did of the electric light, he would freely give three times the amount he now paid for gas to have it in his house, but he

would be sorry to indulge in the luxury to the annoyance of his neighbours.

It had been stated that every person admitted the superiority of the electric light over all other artificial light, but is that so? He thought not; for a few days since a lecture was delivered in that very room by a gentleman who extolled the advantages of candles, and informed his audience that the inventor of the candle would soon celebrate the eve of his hundredth birthday with an illumination of candles, which would eclipse even the electric light. Now, if the figures Mr. Crompton had given were correct, he would certainly implore him, on the ground of humanity, to write at once to that gentleman, and caution him against such a suicidal act as to place himself and friends in such a poisonous atmosphere as would be created by such an illumination.

Mr. R. E. Crompton said his remarks would hardly be a reply to a discussion on his paper, as there had virtually been none. Every one seemed to think that what he had urged on the advantages of electric lighting in regard to health was well known and agreed to, and that in bringing so prominently forward the disadvantages of gas and the older illuminants he had been flogging a dead horse; but the fact was he had written his paper to order, one of the objects of the Conference being to bring very strongly before the public the fact that electric lighting had such enormous advantages; for although many persons might be aware of them, the facts were not known to all, and reiteration might do a great deal of good, and could do no possible harm.

After all, as Mr. Offor had said, the reason why people did not have the electric light was because they would not pay for it at present; but if it were a thoroughly acknowledged fact that in paying for the electric light the public were paying for health at the same time, and that every hour of a man's life spent in a room lighted by gas tended to shorten his life and abridge his powers of work, in time the public might change their minds sufficiently to cause them to spend their money on healthy light, as they were now so largely doing on the sanitary and other arrangements necessary to make healthy houses.

In reply to Colonel Malcolm, he would be indeed flogging a dead horse if he was to keep on reiterating the smallness of the danger due to the electric light as compared with that due to gas, and the matches inseparable from its use. The dangers were in fact infinitesimal; those from fire risks were extremely small if the most ordinary precautions were taken to provide conductors of sufficient section and insulation.

The insurance companies had drawn up rules which were very stringent in respect to fire risks; and if these rules were adhered to they would certainly prevent fires occurring.

The dangers of life were practically confined to those from the use of arc lighting currents of extremely high tension, which would not be used in domestic lighting.

Several speakers had asked the question how it was that electric lighting was not more generally used. This was a very complicated question to answer. No doubt at the time of the formation of the electric light companies, so many of which had since come to grief, the public imagined they would very soon get the electric light supplied in their houses much in the same way as gas is supplied. The causes of their disappointment were so many, and in most cases so distinct from the subject now under discussion, that he would only say that in his opinion the main obstacle to the general distribution of the light was solely that of want of sufficient capital to carry out installations for the supply of electric lighting on a large scale from house to house. Most of the money subscribed to the companies when they were first formed had been spent on experiments, and no doubt in many cases in attempting to perfect inferior or impossible systems. At any rate, there was not sufficient left to carry out the above object. Single self-contained installations were only possible in a limited number of cases, on account of the great difficulty in finding space for the gas or steam engine. In the confined space available in towns it was a matter of great difficulty to make the generating plant perfectly silent and inoffensive to the neighbours. It was quite true that silent gas-engines were not always perfectly silent, although he had succeeded in making them so by taking extra precautions against the communication of noise and vibration.

No doubt there would be some increase in the number of such self-contained installations, but he hoped that the next step would be in the direction of small co-operative installations, six or seven houses being served from one centre. This would make the cost of generating the light considerably less than is the case when each house has its own generating machinery; and if a few such co-operative installations could be got to work he thought it would be the means of greatly restoring the public confidence in electric lighting as a commercial fact; but it must be clearly understood that the electric light could not be supplied at a cheap rate otherwise than on a large scale, and in order to enable the necessary work to be carried out the confidence of the public must return to enable the necessary capital to be raised.

Colonel Malcolm, R.E., said he was by no means an opponent of electricity, as he was only too anxious to get it; but as the Chairman had invited an attack, the only point he could hit upon was the one he knew was commonly urged in uneducated circles, namely, that of danger. He had elicited a very distinct answer to that question, which he hoped might prove of some service.

The Chairman said it was often necessary to enforce ideas which perhaps might be well known in certain quarters, but were not sufficiently generally known; and in fact changes of this sort could only be brought about in that way. They had heard something with regard to wax and tallow candles which would be new, no doubt, to many, for they had been accustomed rather to put wax candles before gas, and to say they would have nothing to do with poisonous gas, but would keep to the wax candles; but Mr. Crompton had shown that the candle was far inferior. He had just answered a question which had been put several times lately, and the same question was put to himself last night—why the electric light was not supplied to the public; why the electric light companies did not make it known that they were ready to supply the light to a small district of a few houses. He was glad, therefore, that Mr. Crompton had informed them that the companies were perfectly ready to supply a small district; and that it was within their power to do so anywhere, provided they had not to cross a street, which was rather a difficulty in the way

at the present moment. It was only the Post Office who had the right to carry wires or convey electricity along the streets; and it was only by special arrangement that powers could be obtained for crossing streets or laying down wires in the street; hence the necessity for a Provisional Order from the Board of Trade before anything of the sort could be carried out. With regard to the question of danger, that would be brought up more prominently in the paper to be read in the afternoon.

The Conference then adjourned for luncheon.

At half-past two the Conference again assembled, when the following paper was read:—

THE PHYSIOLOGICAL BEARING OF ELECTRICITY ON HEALTH.

By Mr. W. H. STONE, M.A., M.B., F.R.C.P.

It is now about two years since I had the pleasure, in conjunction with my colleague Dr. Kilner, of bringing a paper before this Society, in which we tried to lay down some rudimentary basis for physiological measurement of electricity; and I am happy to acknowledge in beginning that the origin of that paper, as it is of this which follows, was due to a former President of the Society, Colonel Webber. He had clearly realised in what very great confusion the whole question of electricity as applied to physiological—I will not say medical—subjects was, and he asked me if I would undertake to do the best I could with it. The former paper was only preliminary, and intended to clear the ground; even in this I cannot for one moment pretend to have reached anything like finality. A few more observations have been made by as accurate measurement as can be obtained, so as to lay, if possible, something like a solid foundation for what has hitherto been entirely built on sand.

Since the date of the former paper I have had the pleasure of reading one before the British Association at Southport; and the editor of *Nature* has kindly inserted three different notes, in which individual points have been brought forward with a view of just showing the line along which I was working, and thus enabling

fellow-workers to keep pace with me. The notes in *Nature* occurred on June 14th and September 13th, 1883, and on May 15th of the present year.

In the excellent practical paper which we had this morning, which was followed by one of the most agreeable discussions I ever heard, a good deal of the ground was cleared which otherwise I should have attempted to cover, and I may therefore take you at once to the physiological relations, and I may say that I shall not speak about lightning accidents. Lightning of itself is a subject so large, and it has been so long known as a dangerous agent, that the whole afternoon would not suffice for describing what happens. I shall only incidentally speak of it when treating of high tension currents. The same thing applies to sight. The injuries to sight, no doubt from electric light, although ultimately due to electricity, depend more on the intensity of the light and the associated motions in space which light carries with it, than on anything which belongs to it as electricity. Therefore, perhaps I shall be excused from taking up that point, and so attempting to treat on more than can be done efficiently in the time allotted. We have in electric lighting two different things, the two extremes of the spectrum—we have incandescent lights, and we have arc lights. The dangers of the incandescent lights are evidently derived from heat vibrations; the dangers of arc lights are as evidently due to actinic and ultra-violet vibrations.

Perhaps I may be allowed to mention a convenient appliance (for this Society always has been, and I hope will long continue to be, a practical Society), in which, with the assistance of Mr. Gardner, of Messrs. Baker's, of High Holborn, I have somewhat modified the usual goggles.

The goggles sometimes have to be used for incandescent lights, and sometimes for arc lights. They have blue fronts to them, which keep out the heat rays very fairly; they have also red sides. When I look at an incandescent light, and my eyes get irritable, and the conjunctivæ inclined to be troublesome, I use the blue goggles; when at an arc light I shut the sides down and use both. I strongly recommend these to electrical engineers.

We may now proceed to speak about the subject of my

syllabus. It is there said that electricity, as at present used, is at once a source of danger, a possible cause of sickness, and a remedy. You will understand that my power of talking of it as a remedy on the present occasion will be very limited; and, even if I had the opportunity, this would not be the place perhaps in which to speak of it. What I shall mainly consider will be, in the intermediate space between the danger and the remedy, certain means of measuring the risks of danger, and obtaining something like a basis for definite facts. Of course the question of danger with electricity, excepting in the single case of lightning, has only arisen within the recollection of most of us, certainly well within mine. I was present as a boy, I am proud to say, at Faraday's lectures and experiments. I served as the subject for some few of them; and all that has occurred since, these enormous currents which we are now making use of, were of course then practically unknown. But while electricity has been making bigger strides than any other physical science, the so-called medical electricity (I abominate the term, for there is no such thing; but I mean electricity as applied to physiology, and therefore to a certain extent to therapeutical pursuits and ends) has marvellously hung back, and it is certainly still in the ante-Faradaic period. For this I have given some reasons: one is in the syllabus, that the knowledge of physics and the knowledge of physiology is rarely united, and is to a certain extent incongruous, I might even say antagonistic. The mind of the physiologist is not the mind of the physicist, and there is occasionally a little heating from friction between the two. This I hope to avoid as much as possible to-day.

There is another reason for it. It is a good thing for the world at large, it is a good thing for science at large, when one very big man comes and occupies a field. He does all the work that can be required. He settles the induction, as Faraday used to say, and then he leaves the rest to the computers. This was done to a great extent for electricity by Faraday himself. But it sometimes happens that a man is too big for his place; and we had in physiology one of those men, Duchenne of Boulogne, whose work has not been properly appreciated up to now. But

this work was not electrical. He experimented with a very rough induction coil; and I recollect him well in the Paris hospitals—a little man, energetic like a Frenchman, not grand to look at, but full of work—trotting about examining all the patients with this curious induction coil of his. It was a very bad coil, but it had the gift of exciting muscles; and by testing patiently and carefully for years a number of muscles individually with small pointed conductors by means of his induction coil, which was practically only an irritator, only an electric needle which he stuck into these muscles, he accomplished a great work. He isolated two or three distinct diseases not isolated before, but which are now well known. That work was physiological, not electrical. That old-fashioned induction coil has been the means of what has been termed electric testing in physiological research. Now that induction coil is a very complicated, a very uncertain, and a perfectly unmeasured source of force. We do not know—at least we have not known—the least in the world what it is doing. We know that it tickles up the muscles into activity, and we know that if they do not respond naturally, it is an indication of some pathological change in the human body. But that is not electrical; it has got the name of electricity without being electricity. As to electricity pure and simple, I am sorry to say I see great evidence that the medical world is somewhat behindhand. First of all I may quote a pamphlet, which is not directly medical, in which the writer states: "Far less was it known—indeed it is only now beginning to be understood—that man himself is a magnet; that his blood and every tissue of his body is pervaded by magnetic influence; that he may be acted upon magnetically by magnets; that in some persons the magnetism disengaged by the contraction of the muscles is sufficient to deflect the needle of the compass" (magnetism, mind—not electricity); "that the health and comfort of each individual, his physical vigour and mental power, depend largely on his magnetic condition, and his relation to the magnetic forces around him." Every one of these statements, speaking strictly, is false; I absolutely and categorically deny that any one of them is true: on the contrary, they are the

very opposite of the truth. Let us ask those who have to deal with Thomson's galvanometers, if they even walked into a room, being magnets, what would become of the Thomson galvanometer?

Then I go a stage further. In a medical periodical the other day I found a review of a work on electrical medicine. The first statement which it begins with, as a sort of flourish of trumpets, is, "There is no mode of measuring alternating currents." I have not been able to recover that extremely foolish statement, and therefore I will not say where I saw it, but see it I undoubtedly did. Only on the 28th of June an article on galvanic batteries for medical purposes appeared. This is written very much more *bona fide* than the others, and I dare say is in the main correct; but I find in this the following statement:—"The resistance of the body varies within wide limits—300 to 100,000 ohms." These are wide limits; I think they are wider limits than the Society of Telegraph-Engineers would be willing to sanction. If you turn that into current or potential, see what would happen to the patient if during the experiment his resistance should vary from 100,000 ohms down to 500: the man would simply be burnt up alive. Therefore it is high time we should pay attention to this. Going a little further, in the same article we find: "Suffice it to say that the rheostats are conveniently made of telegraph wire well insulated in india-rubber, and the electrodes called so because the human body behaves like an electrolyte in the circuit." I did not certainly think electrodes were so called because the human body acts like an electrolyte in the circuit. But this is in the last number of a good paper on electric science. Is it not really too astonishing?

Now, to get something a little more precise. These come under the head of common errors or dangers. Sight you have permitted me to pass over somewhat perfunctorily, as it was alluded to this morning. As to those to life and health, I might begin by stating that it is perfectly clear, from Mr. Crompton's excellent paper, that for the powerful currents we have been using the accidents and dangers have been singularly few; but it does not follow from that that we have a right from our present

immunity entirely to disregard them, or that circumstances may not arise when they would become a very serious matter. Here of course I take my stand; and I shall appeal rather to my friend the excellent President of the College of Physicians for confirmation as a physiologist, when, speaking of the causes of danger, I say it is obvious that they are not only one. When I say only one, I mean the familiar cause of danger which we all know in shock from lightning. An instance of such danger was the unfortunate man at St. Petersburg who, being short-sighted, put his head too near where he was examining the discharge, and it struck and killed him. We know several such cases. The cause of these deaths is obviously shock. There is no *post mortem* appearance found, and they agree with cases of death from concussion of the brain. But, taking them generally, very high tension currents, such as lightning, seem to kill, by a shock affecting the nervous system and brain, instantly. Now, many of the deaths which have taken place have not been instant, and we must go further afield to find the cause of death. Two other causes of death seem to be indicated. There may be actual catalytic action, actual decomposition of the tissues of the human being; that decomposition I have once seen to occur. It takes place of course at one of the poles; in this particular case at the negative pole. Secondly, you may have that wonderful coagulation of the vessels of the body during life which the great Virchow has not only pointed out, but has brought home and made a household word to every physician. Now, under the definition of thrombosis of one of the larger vessels, such as the heart or the lungs, or the large system of arteries or veins, I believe some of the deaths which have taken place from electricity to be included. At any rate, from the very mutilated details which I can collect as to the death at Birmingham, where a foolish player in the orchestra seized hold of the terminals, it seems to me very much like a case of thrombosis. He became livid, and lived about three-quarters of an hour, which in a case of death by lightning would not have occurred. There was apparently a mixture of asphyxial and thrombotic symptoms, which I have no doubt the President of the Royal College of Physicians will recognise. Therefore I think we

must be alive to that as a possible accident. But no doubt most of the cases have been from shock or syncope. The only accidents I have down of which I can get any details are the musical hall case at Birmingham, which I have disposed of, because the papers said the man lived three-quarters of an hour, and therefore it could not have been a death from shock; and then there was the celebrated Paris accident of two foolish boys or men who tried to climb over the wires; there was the Hatfield accident, and then there was the accident on the Russian yacht. If any members of the Society can give me the details as to what were the particular classes of machines employed, and whether the currents were continuous or alternate, what was the tension and also the period at which the unfortunate sufferers lived after receiving the shock, it would be most valuable.

Chief among common errors, let me name imperfect contact. Any person accustomed to electric measurements will see immediately that the very hopeless statement that the body varies from 500 to 100,000 ohms must be a question of contact—it cannot be anything else; and I believe the differences experienced in the early days of medico-electricity have been that the contacts have been to a degree imperfect that the experimenters and all persons concerned did not in the least suspect. Everybody has known for a long time that the skin when dry is a very perfect insulator; in this country less so than in America—in fact, in that dry country insulation is infinitely more perfect than in Europe; and therefore Professor Holtz consents to send his induction machines to America, and will not send any more to England. We live in a fog, and walk about in a mist; we are a kind of aerial fish. In America, if you walk about during the dry season with your boots off and woollen socks, you can light the gas without any matches. In this country I am afraid matches must still be used. We are living in an epidermis of gutta percha; we are small ambulatory Atlantic cables; and, like cables, we certainly can be charged.

The first question which presented itself to me was how to get rid of this delicate, this very thin but singularly perfect insulation, which prevents contact and raises the resistance sometimes

to 100,000 ohms. I thought simply first of making a hole in the patient, and some of my students allowed this. It was also done by Professor John Morgan of Manchester; but it is not agreeable, and on second thoughts I do not think I should allow it on myself again. I used the little hooks used by surgeons, called "serrefines." I passed them through the epidermis, and the actual contact can be got in that way; but the patient objects, and I do not blame him. I have tried oleate of mercury, which is a good conductor, and that answers very well indeed; but, unfortunately, that is transferred into the patient, and mercury is a somewhat powerful agent. Then I thought of strong salt and water; and with strong salt and water I may honestly say I have succeeded. This method was brought before the British Association; but, besides a good conducting and harmless solution, it obviously showed itself to me that through an imperfect conductor like the skin—a variable conductor—the size of the contact must be geometrically infinite as regards any possible current it should have to transmit. That, I think, I have achieved by using one of these long leaden electrodes rolled in surgical fashion round the hand. That, with salt water underneath in some form,—say, a piece of flannel,—acted so well for my small currents that the skin resistance, and the cause of failure as I hold it to be, was brought down, if not to nil, to a minimum. The argument which I should rely upon here would be this. A patient of mine unfortunately died. I had examined him electrically before that sad event, and had got 1,100 ohms resistance by means of lead electrodes wrapped round his feet. After death, when the poor man could suffer no more from my researches, I passed two long silver needles, intended for curing aneurisms, three inches deep into the soles of his feet—the plantar muscles—that is to say, right through the epidermis into the muscular tissues; and, whereas the resistance had been 1,100 ohms through my large electrodes, it was 50 more through these long needles. Of course the 50 more was due to this, that I had been carrying the poles a little higher; the conductor was somewhat shorter, and I was passing the current through a little more human tissue than before, and the 50 ohms very fairly coincided with the extra distance.

This showed that I had reduced the skin resistance, if not to nil, at any rate to a very small quantity. When these views met the eye of an excellent physicist and electrician, Professor Dolbear of Philadelphia, he wrote me a genial, hearty letter, in which he said he should not deny the fact, but he thought a man with his feet wetted in salt water was in an abnormal condition. He deserves great credit for the word "abnormal." There is no doubt about it; we do not all live with our feet wetted in salt water. In the same way the editor of one scientific paper told me if we do wet our feet in hot water the skin swells up and becomes puffed, the pores open, and all sorts of things happen; to which I feel constrained to reply, that if you wash your feet in water and soap every morning this does not occur, but after, say, six weeks abstinence no doubt it will. If the action of water is constantly applied, this disappears. Now we do not want to know the resistance of the skin, but of the tissues. We know that dry skin will run up to any resistance, and so will good insulators; but you do not want to know the resistance of a telegraph line when it is beautiful dry weather, when there is an east wind, no spiders about, nothing to cause contact, no boys flying kites, nothing going wrong—it is the minimum resistance you want to know; and here I believe the physiological point involved is not the maximum, but the absolute minimum. Now when you take the resistance of the body, after getting rid of this imperfect contact, it comes down to very much less than what has been generally given. The 500 ohms which I quoted from this paper would not have been put in a year ago; it is a stolen arrow out of my quiver, because before that I never saw 500 ohms given as the resistance of the human body; but that it can under certain circumstances, especially those of raised temperature, sink to 500, I have not the smallest doubt, because I can give actual demonstration of the fact.

But there is another point that comes out. The resistance of the human body is not equal, or anything like similar and equal, to high tension and low tension currents. If this be true, and it is one of the points I hope to go some little way towards proving, all our physiological work as to currents as physiological stimulants

will have to be revised in the view of this constant being altered. What is the power of these various induction machines? We know only very vaguely; it has never been measured or allowed for. The induction machines of constant current vary in their effects, not only on account of the physiological differences in the body or electrical differences in the current, but because not so much gets through in the one case as in the other. Take any case I have here in my notes. The resistance of a particular patient, suffering from diabetes, using 6 cells of my bichromate battery of about 1·8 volts each, was from foot to foot 1,210 ohms, from right hand to foot 1,350 ohms, and from left hand to foot exactly the same number. To the induction current from this little coil which I have here, which I have measured and know perfectly the electro-motive force, it was, as against 1,210, only 473 ohms. That cannot be an instrumental error. Then from the right hand to the foot, as against 1,350, it was 735. I made it 750, but I was not quite accustomed to the telephone in those days as a means of measuring two currents on a Wheatstone bridge, and I think that was an instrumental error, only amounting, however, to 15 ohms. I have since taken the resistance of a gentleman, an excellent clinical clerk of mine, a very praiseworthy student of St. Thomas's Hospital, and besides those merits he is 6 feet 3 inches high, and weighs 13 stone. From foot to foot his resistance was on the mean of many experiments 930 ohms. From foot to hand, which in his case, taking the external prominence of the ulna and the external malleolus, measures exactly 7 feet, so that I had 7 feet of human conductor, the resistance in a healthy and athletic condition was only 1,027 ohms, many times over. With alternating currents from foot to foot, as against 930 for a mean of 3 consonant observations, it was 650; from foot to hand, against 1,027, I had the mean of 3 observations, none of which differ much, 820. I think that cannot be an instrumental error. I found a trace of this difference in another case where the electro-motive force, with an ordinary battery, was gradually raised, and the same action took place, but in a very much less marked degree. It was a case of dysentery, complicated by what is called tetany, for which I gave the electricity. The man is

perfectly well now, and I meet him every morning; for these experiments are not dangerous, if anything they are salutary. These are his measurements. With two little bichromate cells, 2,500, 2,600, and 2,600 ohms. With 4 cells, that is twice the electro-motive force, 2,100, 2,000, and 2,000, three observations being taken at 10 minutes' interval. With 6 cells, 1,800 and 1,720. With 10 cells, 1,590, 1,450, and 1,440—this was with an ordinary battery, making an alternative current merely by means of the hand. There was always a rise or fall from polarisation, which immediately takes place in the body; but still allowing for there being a constant error in each case, the steady fall of resistance to electro-motive force with these different powers of the battery quite agrees with the enormous fall which takes place when an induction coil is used. In the induction, which is an alternating current, we have the advantage of stopping polarisation, but in doing so a great part of the resistance has disappeared. The body is infinitely more easily traversed, and it is to be feared physiologically that we have lost a great deal of the delicacy of the test. It seems probable that very low tension currents will detect disease earlier than these high speed express electric currents which fly through anything, diseased and healthy tissue alike. Therefore the older method may physiologically prove the more valuable, though here I wish to speak at present with some reserve.

Another point may be named here. I can show that the resistance of the body is much altered in disease, and that it also alters with the metallic impregnation which takes place in certain trades. I have substantiated this with regard to copper, in a copper worker, and found a difference of nearly 300 ohms between the hand with which he held the hammer, which was impregnated with copper, and the other side. I afterwards extracted from his secretion 3 milligrammes of metallic copper. With mercury the resistance goes down very distinctly, and with lead to a less extent, and I am now investigating silver. Silver I can obtain electrolytically from the body; as to its exact diminution of resistance I wish also to speak with reserve.

Then comes the question, How shall we measure this resistance of the human body? and here I must refer to one or two papers

which have appeared in *Nature*. When first beginning, I tried a rotating, and then a metronomic commutator. The most delicate plan, however, especially to a person habituated to play on keyed musical instruments, was to use two commutating keys for the first and second fingers to manipulate, making alternate contact in one or other direction through the body; it was only an instantaneous contact, and, by carefully watching the galvanometer, tolerably trustworthy readings were obtained. They agreed from week to week, from day to day, and from hour to hour. This was better than using any metronomic commutator, because there is a kind of knack when you are used to a galvanometer: you know what it is going to do, and are ready for it. Here is a book full of measurements which agree fairly well, and some of which have been published; they range generally about 1,000 ohms. A strong muscular man, such as Mr. Shackel, one of my clinical clerks, gave 920; and Mr. Todd, equally active but of much smaller stature, gave 940. Apparently the physiological law holds that the muscular size of the limbs to a certain extent compensates for the length of lever, and the resistance of men of different sizes comes in health to very much the same thing. In disease it may differ as much as 300 ohms. I brought before the British Association, at Southport, six cases of *hemiplegia*, or paralysis of one side—three on the left and three on the right. Of course they were selected cases, and the resistance was in the diseased side measurably less than in the healthy, generally about 300 ohms. This, again, I submit, cannot be a mere instrumental error. It must be traced, if it is traced, to physiological facts.

At the meeting at Southport, my friend Professor Oliver Lodge suggested to me, that instead of using a galvanometer and resistance coils, with an ordinary battery, I had better use an induction coil and a telephone. We all know how delicate the telephone is in cases of alternating currents; and shortly afterwards I saw this very pretty instrument of Professor Kohlrausch's named in the *Telegraphic Journal*, and at once sent for it. There, the induction coil is mounted on a short metre bridge; the bridge is only one quarter of a metre long, but the scale underneath the metre is divided so as to be read off directly. You have fixed

resistances of 1, 10, 100, and 1,000 ohms. Those of course practically become multipliers to the metre bridge.

I have been using this instrument; but, although very good for the measurement of fluid resistance, there is no doubt it measures somewhat lower than the ordinary low-tension currents; and it has the disadvantage that, when the 1,000 plug is drawn, so considerable a portion of the induction current passes into the patient that he generally jumps out of his bath and makes strong remarks. Therefore I have been obliged to limit myself to the 100-plug resistance, which sends a good proportion of the current through the senseless instrument, though it a little crowds my measurements. I therefore procured a very beautiful bridge, also designed by Professor Kohlrausch, three metres long, of which the metre wire is wound ten times round a barrel; the edge of the barrel is graduated to 100°, so you read by means of a rotating contact the wire coils, and then take off the fractional parts by a fiducial mark from the graduated edge. It was suggested by Mr. Glazebrook, at a meeting of the Physical Society, that the telephone was too delicate to be used with this instrument. It is so delicate that one single 100th of a rotation of the circumference of the barrel, which has to be multiplied by 10 for the length of the wire, on either side of the minimum sound, is perfectly distinguishable with a little practice. It requires practice and a musical ear. I happen to have an unmusical house physician who cannot make the observations half so well as I. With this I have been executing a number of measurements, and the resistance proves to be entirely different for this instrument and for an ordinary battery; but if that be a difficulty regarded on the electric side, on the other hand it will prove an advantage on the physiological side, because we shall have a delicate new test of the muscular tissue in health and disease, according as it transmits one or the other of these currents. Such distinction is by no means new. It has long been suggested that there is what is called the reaction of degeneration, which has been spoken of by Erb and Westphal, two German physicians, and it has been pretty well accepted now as a medical fact; but it will have to be revised in the light of different conductivity.

The only thing which now remains, before speaking very briefly of what electricity will do, is how shall we obtain a standard of measurement? In this case we know the electromotive force, but if the *Lancet* is right there is no mode at present of measuring alternating currents. There are many persons in this room who can state the contrary of that; and when I wanted to find the means of doing such an impossible thing, I went to the Cantor lectures of our Chairman. There I found a very simple formula which does it in an instant, and which is a formula that has been used by Dr. Hopkinson. Still lower is the ordinary formula for the electrometer charged to a considerable potential.

$$c = \frac{V_1 - V_2}{r_1}$$

$$d = k(V_1 - V_2)\left(V - \frac{V_1 + V_2}{2}\right).$$

There is another method which is more practical for ordinary purposes, namely, the dynamometer. Two years and one month ago I brought this little instrument, made by my own hands in my own little workshop, before the Physiological Society. It is a small dynamometer in which the ordinary heavy coil, instead of being made of copper wire, is made of aluminium wire. That wire, I say distinctly, for a given mass, is far the best conductor existing of electricity, and if a light coil which shall move with a small movement is needed, one had far better use aluminium than any other metal. It was severely criticised by the Physiological Society at Oxford. In the first place, I was told by a member of the firm of Siemens that I could not get contact, and that the contact would be found to fail. This instrument has been knocking about my lecture-room and laboratory for twenty-five months; the contacts are made simply by twisting the wires together, and the variation of resistance is not one-tenth ohm. I was also told that it had been tried for siphon recorders instead of a gold coil, but was not half so good. On that point I leave every one to his own opinion, but I adhere to mine that a light aluminium coil is better than a heavy coil, at any rate for my purpose. I purchased recently this very expensive dynamometer,

also designed by Professor Kohlrausch of Wurtzberg. It is a beautiful instrument: a plate of platinum dipping into a vessel of sulphuric acid makes one contact, the other is a fine silver wire. In mine the contacts are made with a strong silver-gilt wire used for making officers' epaulettes: it is hard wire and keeps its elasticity exceptionally well. No doubt the German instrument measures alternating currents, which the *Lancet* says cannot be done, but it is a long time about it; it is very sluggish. I have to put on a short contact key, then take the reading telescope (also a beautiful piece of mechanism) with metre scale underneath, only with the numbers inverted; it is read off with the telescope at a metre distance, and at about the end of four or five minutes it has gone up to its highest deflection. Of course with this current it goes up by little jerks, and by the time it has done so the battery has run down; and if anybody would tell me of a battery which would stand that treatment I should be deeply obliged to them. Therefore that sluggish heavy copper wire, weighing an ounce or two, will not do. This one, with the despised aluminium, gets up to its full deflection instantly, simply because it has so little moment of inertia, and long before the battery has time to run down I have my readings; and the reading of this scale at one metre's distance with this telescope, with the small induction coil attached to the metre bridge, was on the scale the exact number of the days in a year, viz., 365 millimètres, a deflection abundant for any purposes. Here I had a reading in dynamometer measurement of the electro-motive force between two terminals of this particular coil. This had to be compared with a quadrant electrometer; but, unfortunately, I had not got a quadrant electrometer, nor did I know where to get one. After spending £50 on these instruments you see here, I wrote, hoping the Royal Society would assist me, but I twice received a formal refusal on a printed form. The question was, how was I to get hold of a quadrant electrometer, of which the constants were determined, and by which a poor working physician and physiologist could make a single determination without buying an expensive electrometer, which would cost six months' work to get in order? I think that if such a

Society as this were to interfere in favour of us poor physiologists, and let us have the use of some standard instruments, it would fill up a very decided gap. It is not as if it were wanted on every day of the week, but only three or four times, just to get our constants. I accordingly appealed to my good friends at Cooper's Hill, Professor McLeod, Professor Stocker, and Mr. Gregory. They stood by me magnificently, and these measurements were made last week. At last we got the right determination, which comes as nearly as possible to the electro-motive force when the whole secondary current is allowed to enter the electrometer—about 401 volts. When it is divided on the bridge, if it is equally divided half goes through the patient and half through the fixed coil, so that we are dealing with a total electro-motive force of 40 volts, which gives a deflection on this dynamometer of 365. The deflection in this electrometer was about 1,070.

Though feeling that I ought to finish, I am tempted to say one word about the therapeutic value of electricity. Here we are in a very initiatory stage. I have shown that up to the present our very measurements are not made, and we do not yet know the tools we are dealing with. This Society of Physicists will, I am sure, enforce what I say. Before you begin to work with any tool you ought to know its weight, size, and efficiency, and this we were entirely ignorant of up to now. Then what can we do? There is no doubt that electricity, although it is not life, is very much akin to the principle by which impulses are transmitted in the human body, and therefore its effect both on muscles and nerves is very decided. Hitherto, as regards the curing of any diseases, I fear, more because of our ignorance how to use it than of its want of intrinsic power, we have not achieved any great result. We can do something where muscle is wasted; in cases allied to palsy in its various forms, where the palsy is more or less due to nervous disease, we can get the muscles exercised. I am in the habit of telling my hospital patients that I can give them a two miles' walk while they stay in bed. By attaching an intermittent current to the foot they will have the effect of a two miles' walk without moving out of bed, and the muscles will plump up and get firm and strong; and if the nervous

disease which has caused the affection is only temporary, and is going to fade away, the muscles will not be left in an incompetent state with respect to the renewed nervous stimulus. It is also useful in various nervous diseases. I have known many painful neuralgias yield to a few properly applied currents. In certain diseases of the spine, such as locomotor ataxy and progressive muscular atrophy, you can do a great deal in a curative way.

The eliminative use of electric currents has not been utilised as it should be. I have thus extracted copper, mercury, and silver from the body; and lead has long been got out of the system by this means. The electrical current is a mode of loosening these metals in the system, and then eliminants of a chemical type can be used. For copper I have used sulphate of ammonia, and the excreted copper increased immediately. In the case of copper, sulphate of ammonia answered perfectly. In the case of mercury, iodide of potash has long been known to be efficacious. In the case of silver, I have been using hypo-sulphite of soda. I got the platinum plates well coated with silver and an abundant secretion of hypo-sulphite of soda. I find that agrees perfectly well with the patient.

Beyond this, it is proved also that there is an action of electricity upon that mysterious disease, diabetes. This curious physiological fact I brought before the British Association three years ago. There can be no doubt that the quantity of sugar and the quantity of excretion diminishes exceedingly if you pass a current across what is called the dialectic tract of the brain, namely, floor of the fourth ventricle; unfortunately the influence is transient. Diabetes is an obstinate and persistent disease; and though I am almost certain to be able to produce a result, it evanesces after a week or two. Of course, if I could find any way in that period of pause or rest of lessening the cause—of which we are very ignorant now—of the increased excretion of the abnormal element, sugar, this might come in as an element in a rational and successful cure; but at present, as my old colleague Dr. Ord suggests, the effect is probably on the vaso-motor nerves, due to a power of contracting the calibre of the vessels by the action on the vaso-motor nerves. There can be no doubt that

the floor of the fourth ventricle is in some way connected with this morbid excretion of sugar; of the intermediate stages we know very little. A fact has come to my knowledge a few days since, which very strongly confirms the idea that there may be some vaso-motor action when a current is passed through the base of the brain. It comes to me from another distinguished member of this Society, Colonel Bolton. He tells me he finds a moderate continuous current passed through the head from the back of the neck to one of the hands, when you are sleepless from overwork or any other cause at night, will act as an efficient hypnotic. If that is a fact, and it was given to me entirely spontaneously without any leading up to, purely and simply as an observed fact, we have got hold of something very valuable. It agrees extremely well with the effects known in the case of hypnotics like chloral. We all know that the condition of the brain during sleep is anæmic, the supply of blood is lessened, function becomes to a certain extent diminished, and the body falls off into the condition of repose which is the twin sister of death. No doubt here are joined together the two terminals of what I began with. This is the explanation then. The vaso-motor action—the contraction of the arteries at the base of the brain—is the cause of many fatal accidents; but if we can also say that it is the cause of the beneficent action of refreshing sleep, we shall to a certain extent redeem the character of the agent, and show that it does some good to compensate for its occasional evil results.

The Chairman said they had had a most interesting lecture from Dr. Stone, who had thrown out some practical points which would not be accepted without question in some cases, and on other points he had directly applied to electricians to throw out ideas or give him information. For instance, he had asked them to tell him of a kind of battery which he could use for certain purposes, and also what kind of instrument would be best for the special investigations he had in hand.

Mr. Latimer Clark said he was in communication with the officers of the Russian sloop "Livadia," on which the accident occurred, and he would endeavour to get some detailed informa-

tion on the subject, and communicate it to Dr. Stone. The subject of the application of electricity to medicine has appeared in all ages an important one, and no doubt many had wondered why so little practical success had hitherto been achieved. He felt that now perhaps for the first time they were in the right channel, for Dr. Stone was dealing with the subject in a thoroughly scientific manner and spirit, and if anything could be done to apply medical electricity to the benefit of mankind, Dr. Stone was likely to achieve it. Amongst other things, Dr. Stone had spoken of popular errors, and he was glad to hear he had touched upon one that was very common. There seemed to be a sort of vague idea that currents of high tension were painful and dangerous on account of their high potential, but that was a popular error which should be exploded. He believed the human frame was precisely like a galvanometer—that the physiological influence of an electric current was solely proportionate to the quantity of electricity which actually passed through the body, and not at all to its tension. Of course high tension caused more electricity to pass through the body, and Dr. Stone produced the same result by giving better conductors to the hands and feet, but he conceived they should regard the human frame as a galvanometer, and that the physiological effects were proportionate to the quantity passing, and not to the potential of the current which passed.

Dr. KILNER: There is one point in Dr. Stone's valuable paper to which I must take exception, and that is the method he has been describing for obtaining the resistance of the human body. He has measured the resistance either from hand to hand or from foot to foot, or from hand to foot, thus including a large portion of the body, without any attempt to localise the resistance of the different parts. Although he has found the resistances taken in this manner vary from time to time (and supposing, for an example, the resistance is measured from hand to foot), he is unable to say whether the change is due to an alteration of resistance that has occurred throughout, or only in the arm, or in the trunk, or in the leg.

For the last two or three years I have been in the habit of

measuring the resistance of the human body, only in a different manner, aiming at obtaining the local resistance rather than that offered by a large extent of the body, because by so doing a more useful knowledge is obtained, as the resistance of a diseased limb not only very frequently differs from what it would be in health, but that under certain circumstances the local alteration of the resistance has enabled me to give a more certain prognosis than I could have done without. The method I employ was fully described last year in the *Lancet*, and is, leaving out the details, as follows:—I place the two electrodes at a convenient distance apart, and measure the current by means of a vertical mirror galvanometer as quickly as possible, and then place the eloctrodes at exactly half the distance apart, and again measure the current, using the same battery. From this, if the area of the electrodes be known, not only can the resistance of the epidermis, but also that offered by the subjacent tissues between the two electrodes, be calculated. Another advantage accrues from this method, viz., that as the resistance is altered after the passage of the current from what it was previously, the state of contraction of the local blood-vessels can be determined, because the current dilates them, and as the blood is the best conductor of all the animal tissues, the resistance will decrease in direct proportion to the increase of the amount of blood to the part.

This leads us to another point just touched upon by Dr. Stone, namely, the effect of the current upon the circulation. I have this day finished a paper upon the subject, and may state that there is a great difference between the constant current and one obtained from an induction coil, inasmuch as the latter exerts its power chiefly upon the general circulation by lowering the arterial tension, no matter where the electrodes may be placed, while the former is almost entirely local in its action.

Mr. Beeman said the accident at Hatfield took place nearly half a mile away from the machine, and, though no doubt the coroner's jury brought it in that the man died from electric shock, he believed that if Dr. Stone communicated with the medical officer it would be found that the man was suffering from heart disease, and was six yards away from the wire which was supposed

to have killed him. There was not the slightest mark on his body to indicate that he had touched the wires at all.

Mr. GREENHILL said if a man were suffering from disease of the heart and got a sudden shock it was very probable he would die, and his death would be so recorded, usually speaking; but if he happened to be near a dynamo-machine, the papers would record that he was killed by a shock of electricity.

Dr. STONE said he had a newspaper report which bore out very much Mr. Greenhill's remarks, for, though attributed to electricity, it was an obvious case of heart disease.

The CHAIRMAN, in proposing a vote of thanks to Dr. Stone, said he must make one remark with regard to the accuracy of one of the instruments referred to. He was not sure that electricians would be quite satisfied with the accuracy of what was put forward by Dr. Stone, viz., that resistance could be measured to within $\frac{1}{10}$ of an ohm by the instrument in which the aluminum wire was used. Nowadays electricians would not be satisfied with anything which did not give the resistance in ohms carried to several places of decimals.

Dr. STONE, in reply, said he should be very much obliged to Mr. Latimer Clark if he would kindly get him the information in regard to the accident. It seemed to him that rapid fluctuations of current created the danger, not the actual quantity of current, and even then the action was more marked on the unstriped or involuntary muscles than on the voluntary muscle fibre. Most of his experiments had been conducted conjointly with Dr. Kilner, who had worked with him now for several years, and therefore his experience naturally coincided. As regards Mr. Walker, he agreed with some things he said, but did not quite agree in others. It was quite impossible to attempt to distinguish between the tissues of the body when a current was passing through them all. It passed through them indifferently, and you could not separate it, or say that the blood, the muscle, or the nerve conducted so much. As far as they knew, the nerve, to ordinary electricity, instead of being what it should be, a good conductor, was a remarkably bad one; muscle was much better; and water, simple serous fluid, or saline solutions were better than all of them. He

thought that the body followed the law, as regards heat, more of solid than fluid conductors: where dropsy took place resistance went down to half, the dropsical fluid being a much better conductor than healthy muscle, bone, or tendon. As to the movements of a conducting fluid, it did not move in a magnetic field; and nobody but the author of the pamphlet he had quoted suggested that there was in the interior of the body a magnetic field of any appreciable force. He certainly was not aware of it, or of the influence of magnets in his neighbourhood, nor had he ever been able to influence himself by the neighbourhood of magnets. He had tried the experiment on a large scale, and it was now being repeated by a society at his laboratory, but excepting the imagined sight of flames by some hysterical girls, nobody had been able to point to any effect produced by magnets whatever. He had used a battery of fifty 3-quart cells and a large electro-magnet, and had lain down on the pillow with his head between the poles of this enormous magnet, but though it was competent to rotate a ray of light through a number of degrees, it could not rotate him through one.

THE LIBRARY.

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By Alfred J. Frost, *Librarian.*

(*Works marked with an Asterisk* (*) *have been purchased.*)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD FORWARD COPIES OF THEIR WORKS TO THE LIBRARY, AS SOON AS POSSIBLE AFTER PUBLICATION.

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OBITUARY.

SIR WILLIAM SIEMENS.

Written by J. MUNRO, at the request of the Council.

CHARLES WILLIAM SIEMENS was born on April 4, 1823, at the little village of Lenthe, about eight miles from Hanover, where his father, Mr. Ferdinand Siemens, farmed an estate belonging to the Crown. His mother's maiden name was Eleonore Deichmann; and William, or Wilhelm, was the eighth son of a family of fourteen, several of whom have distinguished themselves in scientific pursuits. Of these, Ernst Werner Siemens, the fourth child, and now the famous electrician of Berlin, was associated with William in many of his inventions; Fritz, the ninth child, is the head of the well-known Dresden glass works; and Carl, the tenth child, is chief of the equally well-known electrical works at St. Petersburgh. Several of the family died young; others remained in Germany; but the enterprising spirit, natural to them, led most of the sons abroad—Walter, the twelfth child, dying at Tiflis as the German Consul there, and Otto, the fourteenth child, also dying at the same place. It would be difficult to find a more remarkable family in any age or country.

Charles William received his education at the Gymnasium in Lübeck, the Polytechnic School at Magdeburg (city of the memorable burgomaster, Otto von Guerické), and at the University of Göttingen, which he entered in 1841, while in his eighteenth year. Here he attended the chemical lectures of Woehler, the discoverer of organic synthesis, and of Professor Himly, the well-known physicist, who was married to Siemens's eldest sister, Mathilde. With a year at Göttingen, during which he laid the basis of his theoretical knowledge, the academical training of Siemens came to an end, and he entered practical life in the engineering works of Count Stolberg. At the University he had been instructed in mechanical

laws and designs; here he learned the nature and use of tools and the construction of machines. But as his University career at Göttingen lasted only about a year, so did his apprenticeship at the Stolberg Works. In this short time, however, he probably reaped as much advantage as a duller pupil during a much longer term.

Young Siemens appears to have been determined to push his way forward. In 1841, his brother Werner obtained a patent in Prussia for electro-silvering and gilding; and in 1843 Charles William came to England to try and introduce the process here. In his address on "Science and Industry," delivered before the Birmingham and Midland Institute in 1881, while the Paris Electrical Exhibition was running, Sir William gave a most interesting account of his experiences during that first visit to the country of his adoption.

"When," said he, "the electrotype process first became known, it excited a very general interest; and although I was only a young student at Göttingen, under twenty years of age, who had just entered upon his practical career with a mechanical engineer, I joined my brother, Werner Siemens, then a young lieutenant of artillery in the Prussian service, in his endeavours to accomplish electro-gilding; the first impulse in this direction having been given by Professor C. Himly, then of Göttingen. After attaining some promising results, a spirit of enterprise came over me, so strong that I tore myself away from the narrow circumstances surrounding me, and landed at the east end of London with only a few pounds in my pocket and without friends, but with an ardent confidence of ultimate success within my breast.

"I expected to find some office in which inventions were examined into, and rewarded if found meritorious, but no one could direct me to such a place. In walking along Finsbury Pavement, I saw written up in large letters, 'So and so' (I forget the name), 'Undertaker,' and the thought struck me that this must be the place I was in quest off; at any rate, I thought that a person advertising himself as an 'undertaker' would not refuse to look into my invention with a view of obtaining for me the sought-for recognition or reward. On entering the place I soon

convinced myself, however, that I came decidedly too soon for the kind of enterprise here contemplated, and, finding myself confronted with the proprietor of the establishment, I covered my retreat by what he must have thought a very lame excuse. By dint of perseverance I found my way to the patent office of Messrs. Poole & Carpmael, who received me kindly, and provided me with a letter of introduction to Mr. Elkington. Armed with this letter, I proceeded to Birmingham, to plead my cause before your townsman.

"In looking back to that time, I wonder at the patience with which Mr. Elkington listened to what I had to say, being very young, and scarcely able to find English words to convey my meaning. After showing me what he was doing already in the way of electro-plating, Mr. Elkington sent me back to London in order to read some patents of his own, asking me to return if, after perusal, I still thought I could teach him anything. To my great disappointment, I found that the chemical solutions I had been using were actually mentioned in one of his patents, although in a manner that would hardly have sufficed to enable a third person to obtain practical results.

"On my return to Birmingham I frankly stated what I had found, and with this frankness I evidently gained the favour of another townsman of yours, Mr. Josiah Mason, who had just joined Mr. Elkington in business, and whose name, as Sir Josiah Mason, will ever be remembered for his munificent endowment of education. It was agreed that I should not be judged by the novelty of my invention, but by the results which I promised, namely, of being able to deposit with a smooth surface 30 dwt. of silver upon a dish-cover, the crystalline structure of the deposit having theretofore been a source of difficulty. In this I succeeded, and I was able to return to my native country and my mechanical engineering a comparative Crœsus.

"But it was not for long, as in the following year (1844) I again landed in the Thames with another invention, worked out also with my brother, namely, the chronometric governor, which, though less successful, commercially speaking, than the first, obtained for me the advantage of bringing me into contact with

the engineering world, and of fixing me permanently in this country. This invention was in course of time applied by Sir George Airy, the then Astronomer-Royal, for regulating the motion of his great transit and touch-recording instrument at the Royal Observatory, where it still continues to be employed.

"Another early subject of mine, the anastatic printing process, found favour with Faraday, 'the great and the good,' who made it the subject of a Friday evening lecture at the Royal Institution. These two circumstances, combined, obtained for me an entry into scientific circles, and helped to sustain me in difficulty, until, by dint of a certain determination to win, I was able to advance step by step up to this place of honour, situated within a gunshot of the scene of my earliest success in life, but separated from it by the time of a generation. But notwithstanding the lapse of time, my heart still beats quick each time I come back to the scene of this, the determining incident of my life."

The "anastatic" process, described by Faraday in 1845, and partly due to Werner Siemens, was a method of reproducing printed matter by transferring the print from paper to plates of zinc. Caustic baryta was applied to the printed sheet to convert the resinous ingredients of the ink into an insoluble soap, the stearine being precipitated with sulphuric acid. The letters were then transferred to the zinc by pressure, so as to be printed from. The process, though ingenious and of much interest at the time, has long ago been superseded by photographic methods.

Even at this time Siemens had several irons in the fire. Besides the printing process and the chronometric governor, which operated by the differential movement between the engine and a chronometer, he was occupied with some minor improvements at Hoyle's Calico Printing Works. He also engaged in railway works from time to time; and in 1846 he brought out a double cylinder air-pump, in which the two cylinders are so combined, that the compressing side of the first and larger cylinder communicated with the suction side of the second and smaller cylinder, and the limit of exhaustion was thereby much extended. The invention was well received at the time, but is now almost forgotten.

Siemens had been trained as a mechanical engineer, and, although he became an eminent electrician in later life, his most important work at this early stage was non-electrical; indeed, the greatest achievement of his life was non-electrical, for we must regard the regenerative furnace as his *magnum opus*. Though in 1847 he published a paper in Liebig's *Annalen der Chemie* on the "Mercaptan of Selenium," his mind was busy with the new ideas upon the nature of heat which were promulgated by Carnot, Clayperon, Joule, Clausius, Mayer, Thomson, and Rankine. He discarded the older notions of heat as a substance, and accepted it as a form of energy. Working on this new line of thought, which gave him an advantage over other inventors of his time, he made his first attempt to economise heat, by constructing in 1847, at the factory of Mr. John Hick, of Bolton, an engine of four horse-power, having a condenser provided with regenerators, and utilising superheated steam. The use of superheated steam was, however, attended with many practical difficulties, and the invention was not entirely successful, but it embraced the elements of success; and the Society of Arts, in 1850, acknowledged the value of the principle, by awarding Mr. Siemens a gold medal for his regenerative condenser. Various papers read before the Institution of Mechanical Engineers, the Institution of Civil Engineers, or appearing in *Dingler's Journal* and the *Journal of the Franklin Institute* about this time, illustrate the workings of his mind upon the subject. That read in 1853, before the Institution of Civil Engineers, "On the Conversion of Heat into Mechanical Effect," was the first of a long series of communications to that learned body, and gained for its author the Telford premium and medal. In it he contended that a perfect engine would be one in which all the heat applied to the steam was used up in its expansion behind a working piston, leaving none to be sent into a condenser or the atmosphere, and that the best results in any actual engine would be attained by carrying expansion to the furthest possible limit, or, in practice, by the application of a regenerator. Anxious to realise his theories further, he constructed a twenty horse-power engine on the regenerative plan, and exhibited it at the Paris Universal Exhibition of 1855;

but, not realising his expectations, he substituted for it another of seven horse-power, made by M. Farcot, of Paris, which was found to work with considerable economy. The use of superheated steam, however, still proved a drawback, and the Siemens engine has not been extensively used.

On the other hand, the Siemens water-meter, which he introduced in 1851, has been very widely used, not only in this country, but abroad. It acts equally well under all variations of pressure, and with a constant or an intermittent supply.

Meanwhile his brother Werner had been turning his attention to telegraphy, and the correspondence which never ceased between the brothers kept William acquainted with his doings. In 1844, Werner, then an officer in the Prussian army, was appointed to a berth in the artillery workshops of Berlin, where he began to take an interest in the new art of telegraphy. In 1845, Werner patented his dial and printing telegraph instruments, which came into use all over Germany, and introduced an automatic alarm on the same principle. These inventions led to his being made, in 1846, a member of a commission in Berlin for the introduction of electric telegraphs instead of semaphores. He advocated the use of gutta percha, then a new material, for the insulation of underground wires, and in 1847 designed a screw-press for coating the wires with the gum rendered plastic by heat. The following year he laid the first great underground telegraph line from Berlin to Frankfort-on-the-Main, and soon afterwards left the army to engage with Mr. Halske in the management of a telegraph factory which they had conjointly established in 1847. Two years later, Mr. Halske and William Siemens founded in London the house of Siemens, Halske, & Co., which developed in course of time into the firm of Siemens Brothers, and was recently transformed into a limited liability company.

In 1850, William Siemens became a naturalised Englishman, and from this time forward took an active part in the progress of English engineering and telegraphy. He devoted a great part of his time to electrical invention and research; and the number of telegraph apparatus of all sorts, telegraph cables, land-lines, and their accessories, which have emanated from the Siemens

Telegraph Works has been remarkable. The engineers of this firm have been pioneers of the electric telegraph in all quarters of the globe, both by land and sea. The most important aërial line erected by the firm was the Indo-European telegraph line, through Russia and Persia, to India. The North China cable, the Platino-Brazileira, and the Direct United States cable were laid by the firm, the latter in 1874. So also was the French Atlantic cable, and the two Jay Gould Atlantic cables. At present the manufacture and laying of the Bennett-Mackay Atlantic cables is being proceeded with at the company's works, Charlton. Some idea of the extent of this manufactory may be gathered from the fact that it gives employment to some 2,000 men. All branches of electrical work are followed out in its various departments, including the construction of dynamos and electric lamps.

On July 23, 1859, Siemens was married to Anne Gordon, youngest daughter of Mr. Joseph Gordon, Writer to the Signet, Edinburgh.

Although much engaged in the advancement of telegraphy, he was also occupied with his favourite idea of regeneration. The regenerative gas furnace, originally invented in 1848, was perfected by him and his brother Frederick during many succeeding years. The difficulties overcome in the development of this invention were enormous, but the final triumph was complete.

The principle of this furnace consists in utilising the heat of the products of combustion to warm up the gaseous fuel and air which enters the furnace. This is done by making these products pass through brickwork chambers which absorb their heat and communicate it to the gas and air currents going to the flame. An extremely high temperature is thus obtained, and the furnace has in consequence been largely used in the manufacture of glass and steel.

Before the introduction of this furnace, attempts had been made to produce cast-steel without the use of a crucible, that is to say, on the "open hearth" of the furnace. Reamur was probably the first to show that steel could be made by fusing malleable iron with cast-iron. Heath patented the process in

1845; and a quantity of cast-steel was actually prepared in this way, on the bed of a reverberatory furnace, by Sudre, in France, during the year 1860. But the furnace was destroyed in the act; and it remained for Siemens, with his regenerative furnace, to realise the object. In 1862, Mr. Charles Atwood, of Tow Law, agreed to erect such a furnace, and give the process a fair trial; but although successful in producing the steel, he was afraid its temper was not satisfactory, and discontinued the experiment. Next year, however, Siemens, who was not to be disheartened, made another attempt with a large furnace erected at the Montluçon Works, in France, where he was assisted by the late M. le Chatellier, Inspecteur-Général des Mines. Some charges of steel were produced; but here again the roof of the furnace melted down, and the company which had undertaken the trials gave them up. The temperature required for the manufacture of the steel was higher than the melting point of most fire-bricks. Further endeavours also led to disappointments; but in the end the inventor was successful. He erected experimental works at Birmingham, and gradually matured his process until it was so far advanced that it could be trusted to the hands of others. Siemens used a mixture of cast-steel and iron ore to make the steel; but another manufacturer, M. Martin, of Sireuil, in France, developed the older plan of mixing the cast-iron with wrought-iron scrap. While Siemens was improving his means at Birmingham, Martin was obtaining satisfactory results with a regenerative furnace of his own design; and at the Paris Exhibition of 1867 samples of good open-hearth steel were shown by both manufacturers. In England the process is now generally known as the "Siemens-Martin," and on the Continent as the "Martin-Siemens" process.

The regenerative furnace is the greatest single invention of Charles William Siemens. Owing to the large demand for steel for engineering operations, both at home and abroad, it proved exceedingly remunerative. Extensive works for the application of the process were erected at Landore, where Siemens prosecuted his experiments on the subject with unfailing ardour, and, among other things, succeeded in making a basic brick for the lining of his furnaces, which withstood the intense heat fairly well.

The process in detail consists in freeing the bath of melted pig-iron from excess of carbon, by adding broken lumps of pure hematite or magnetite iron ore. This causes a violent boiling, which is kept up until the metal becomes soft enough, when it is allowed to stand to let the metal clear from the slag which floats in scum upon the top. The separation of the slag and iron is facilitated by throwing in some lime from time to time. Spiegel, or specular iron, is then added; about 1 per cent. more than in the scrap process. From 20 to 24 cwt. of ore are used in a 5-ton charge; and about half the metal is reduced and turned into steel, so that the yield in ingots is from 1 to 2 per cent. more than the weight of pig and spiegel iron in the charge. The consumption of coal is rather larger than in the scrap process, and is from 14 to 15 cwt. per ton of steel. The two processes of Siemens and Martin are often combined, both scrap and ore being used in the same charge; the latter being valuable as a tempering material.

At present there are several large works engaged in manufacturing the Siemens-Martin steel in England, namely, the Landore, the Parkhead Forge, those of the Steel Company of Scotland, of Messrs. Vickers & Co., Sheffield, and others. These produced no less than 340,000 tons of steel during the year 1881, and last year the total out-put had risen to half a million tons. In 1876, the British Admiralty built two ironclads, the "Mercury" and "Iris," of Siemens-Martin steel, and the experiment proved so satisfactory that this material only is now used in the Royal Dockyards for the construction of hulls and boilers. Moreover, the use of it is gradually extending in the mercantile marine. Contemporaneous with his development of the open-hearth process, William Siemens introduced the rotary furnace for producing wrought-iron direct from the ore without the need of puddling.

The fervent heat of the Siemens furnace led the inventor to devise a novel means of measuring high temperatures, which illustrates the value of a broad scientific training to the inventor, and the happy manner in which William Siemens, above all others, turned his varied knowledge to account, and brought the

facts and resources of one science to bear upon another. As early as 1860, while engaged in testing the conductor of the Malta to Alexandria telegraph cable, then in course of manufacture, he was struck by the increase of resistance in metallic wires, occasioned by a rise of temperature, and the following year he devised a thermometer based on the fact, which he exhibited before the British Association at Manchester. Mathiessen and others have since enunciated the law according to which this rise of resistance varies with rise of temperature; and Siemens has further perfected his apparatus, and applied it as a pyrometer to the measurement of furnace fires. It forms in reality an electric thermometer, which will indicate the temperature of an inaccessible spot. A coil of platinum or platinum-alloy wire is enclosed in a suitable fire-proof case, and put into the furnace of which the temperature is wanted. Connecting wires, properly protected, lead from the coil to a differential voltameter, so that, by means of the current from a battery circulating in the system, the electric resistance of the coil in the furnace can be determined at any moment. Since this resistance depends on the temperature of the funrace, the temperature can be found from the resistance observed. The instrument formed the subject of the Bakerian lecture for the year 1871.

Siemens's researches on this subject, as published in the *Journal of the Society of Telegraph Engineers* (Vol. I., p. 123, and Vol. III., p. 297), included a set of curves graphically representing the relation between temperature and electrical resistance in the case of various metals.

The electric pyrometer, which is perhaps the most elegant and original of all William Siemens's inventions, is also the link which connects his electrical with his metallurgical researches. His invention ran in two great grooves, one based upon the science of heat, the other based upon the science of electricity; and the electric thermometer was, as it were, a delicate cross-coupling which connected both. Siemens might have been two men, if we are to judge by the work he did; and either half of the twin-career he led would of itself suffice to make an eminent reputation.

The success of his metallurgical enterprise no doubt reacted

on his telegraphic business. The making and laying of the Malta to Alexandria cable gave rise to researches on the resistance and electrification of insulating materials under pressure, which formed the subject of a paper read before the British Association in 1863. The effect of pressure up to 300 atmospheres was observed, and the fact elicited that the inductive capacity of gutta percha is not affected by increased pressure, whereas that of india-rubber is diminished. The electrical tests employed during the construction of the Malta and Alexandria cable, and the insulation and protection of submarine cables, also formed the subject of a paper which was read before the Institution of Civil Engineers in 1862.

It is always interesting to trace the necessity which directly or indirectly was the parent of a particular invention; and in the great importance of an accurate record of the sea-depth in which a cable is being laid, together with the tedious and troublsome character of ordinary sounding by the lead-line, especially when a ship is actually paying out cable, we may find the requirements which led to the invention of the "bathometer," an instrument designed to indicate the depth of water over which a vessel is passing without submerging a line. The instrument was based on the ingenious idea that the attractive power of the earth on a body in the ship must depend on the depth of water interposed between it and the sea-bottom; being less as the layer of water was thicker, owing to the lighter character of water as compared with the denser land. Siemens endeavoured to render this difference visible, by means of mercury contained in a chamber having a bottom extremely sensitive to the pressure of the mercury upon it, and resembling in some respects the vaccuous chamber of an aneroid barometer. Just as the latter instrument indicates the pressure of the atmosphere above it, so the bathometer was intended to show the pull of the earth below it; and experiment proved, we believe, that for every 1,000 fathoms of sea-water below the ship, the total gravity of the mercury was reduced by $\frac{1}{3200}$ part. The bathometer, or attraction-meter, was brought out in 1876, and exhibited at the Loan Exibition in South Kensington. The elastic bottom of the mercury chamber was

supported by volute springs which, always having the same tension, caused a portion of the mercury to rise or fall in a spiral tube of glass, according to the variations of the earth's attraction. The whole was kept at an even temperature, and correction was made for barometric influence. Though of high scientific interest, the apparatus appears to have failed at the time from its very sensitiveness; the waves on the surface of the sea having a greater disturbing action on its readings than the change of depth. Siemens took a great interest in this very original machine, and also devised a form applicable to the measurement of heights. Although he laid the subject aside for some years, he ultimately took it up again, in hopes of producing a practical apparatus which would be of immediate service in the cable expeditions of the s.s. "Faraday."

This admirable cable steamer was built for Messrs. Siemens Brothers by Messrs. Mitchell & Co., at Newcastle. The designs were mainly inspired by Siemens himself; and after the "Hooper," now the "Silvertown," she was the second ship expressly built for cable purposes. All the latest improvements that electric science and naval engineering could suggest were in her united. With a length of 360 feet, a width of 52 feet, and a depth of 36 feet in the hold, she was fitted with a rudder at each end, either of which could be locked when desired, and the other brought into play. Two screw propellers, actuated by a pair of compound engines, were the means of driving the vessel, and they were placed at a slight angle to each other, so that when the engines were worked in opposite directions the "Faraday" could turn completely round in her own length. Moreover, as the ship could steam forwards or backwards with equal ease, it became unnecessary to pass the cable forward before hauling it in, if a fault were discovered in the part submerged: the motion of the ship had only to be reversed, the stern rudder fixed, and the bow rudder turned, while a small engine was employed to haul the cable back over the stern drum which had been used a few minutes before to pay it out.

The first expedition of the "Faraday" was the laying of the direct United States cable in the winter of 1874, a work which,

though interrupted by stormy weather, was resumed and completed in the summer of 1875. She has been engaged in laying several Atlantic cables since, and has been fitted with the electric light, a resource which has proved of the utmost service, not only in facilitating the night operations of paying out, but in guarding the ship from collision with icebergs in foggy weather off the North American coast.

Mention of the electric light brings us to an important act of the inventor, which, though done on behalf of his brother Werner, was pregnant with great consequences. This was his announcement, before a meeting of the Royal Society, held on February 14, 1867, of the discovery of the principle of reinforcing the field magnetism of magneto-electric generators by part or the whole of the current generated in the revolving armature—a discovery on which the present dynamo-electric machines, now so much used for producing electric light and effecting the transmission of power to a distance by means of the electric current. By a curious coincidence the same principle was enunciated by Sir Charles Wheatstone at the very same meeting; while a few months previously Mr. S. A. Varley had applied for a patent in this country, in which the same idea was set forth. The claims of these three inventors to priority in the discovery are, however, anticipated by at least one other investigator, Herr Sören Hjorth, believed to be a Dane by birth, and still remembered by a few living electricians, though forgotten by the scientific world at large, until his neglected English patent was recently dug out of the musty archives of the British Patent Office and brought into the light.

The announcement of Siemens and Wheatstone came at an apter time than Hjorth's, and was more conspicuously made. Above all, in the affluent and enterprising hands of the brothers Siemens, it was not suffered to lie sterile, and the Siemens dynamo-electric machine was its offspring. This machine, as is well known, differs from those of Gramme and Paccinotti chiefly in the longitudinal winding of the armature, and it is unnecessary to describe it here. It has been adapted by its inventors to all kinds of electrical work, electrotyping, telegraphy, electric lighting, and the propulsion of vehicles.

The first electric tramway run at Berlin in 1879 was followed by another at Düsseldorf in 1880, and a third at Paris in 1881. With all of these the name of Werner Siemens was chiefly associated; but William Siemens had also taken up the matter, and established at his house near Redhill an arrangement of dynamos and waterwheel, by which the power of a neighbouring stream was made to light the house, cut chaff, turn washing-machines, and perform other household duties. More recently the construction of the Portrush and Giant's Causeway electric railway engaged his attention; and this, the first work of its kind in the United Kingdom, and to all appearance the pioneer of many similar lines, was one of his very last undertakings.

In the recent development of electric lighting, William Siemens, whose fame had been steadily growing, was a recognised leader, although he himself made no great discoveries therein. As a public man and a manufacturer of great resources, his influence in assisting the introduction of the light has been immense. The number of Siemens machines and Siemens electric lamps, together with measuring instruments such as the Siemens electro-dynamometer, which has been supplied to different parts of the world by the firm of which he was the head, is very considerable, and probably exceeds that of any other manufacturer, at least in this country.

Employing a staff of assistants to develop many of his ideas, Dr. Siemens was able to produce a great variety of electrical instruments for measuring and other auxiliary purposes, all of which bear the name of his firm, and have proved exceedingly useful in a practical sense.

Among the most remarkable of Siemens's investigations were his experiments on the influence of the electric light in promoting the growth of plants, carried out during the winter of 1880 at his country residence, Tunbridge Wells. These experiments showed that plants do not require a period of rest, but continue to grow if light and other necessaries are supplied to them. Siemens enhanced the daylight, and, as it were, prolonged it through the night by means of arc lamps, with the result of forcing excellent fruit and flowers to their maturity before the natural time in this climate.

While Siemens was testing the chemical and life-promoting influence of the electric arc light, he was also occupied in trying its temperature and heating power with an "electric furnace," consisting of a plumbago crucible having two carbon electrodes entering it in such a manner that the voltaic arc could be produced within it. He succeeded in fusing a variety of refractory metals in a comparatively short time: thus, a pound of broken files was melted in a cold crucible in thirteen minutes, a result which is not surprising when we consider that the temperature of the voltaic arc, as measured by Siemens and Rosetti, is between 2,000° and 3,000° centigrade, or about one-third that of the probable temperature of the sun. Sir Humphry Davy was the first to observe the extraordinary fusing power of the voltaic arc, but Siemens first applied it to a practical purpose in his electric furnace.

Always ready to turn his inventive genius in any direction, the introduction of the electric light, which had given an impetus to improvement in the methods of utilising gas, led him to design a regenerative gas lamp, which is now employed on a small scale in this country, either for street lighting or in class-rooms and public halls. In this burner, as in the regenerative furnace, the products of combustion are made to warm up the air and gas which go to feed the flame, and the effect is a full and brilliant light with some economy of fuel. The use of coal-gas for heating purposes was another subject which he took up with characteristic earnestness, and he advocated for a time the use of gas stoves and fires in preference to those which burn coal, not only on account of their cleanliness and convenience, but on the score of preventing fogs in great cities, by checking the discharge of smoke into the atmosphere. He designed a regenerative gas and coke fireplace, in which the ingoing air was warmed by heat conducted from the back part of the grate; and by practical trials in his own office, calculated the economy of the system. The interest in this question, however, died away after the close of the Smoke Abatement Exhibition; and the experiments of Mr. Aiken, of Edinburgh, showed how futile was the hope that gas fires would prevent fogs altogether. They might indeed ameliorate the noxious character

of a fog by checking the discharge of soot into the atmosphere; but Mr. Aiken's experiments showed that particles of gas were in themselves capable of condensing the moisture of the air upon them. The great scheme of Siemens for making London a smokeless city, by manufacturing gas at the coal-pit and leading it in pipes from street to street, would not have rendered it altogether a fogless one, though the coke and gas fires would certainly have reduced the quantity of soot launched into the air. Siemens's scheme was rejected by a Committee of the House of Lords, on the somewhat mistaken ground that if the plan were as profitable as Siemens supposed, it would have been put in practice long ago by private enterprise.

From the problem of heating a room, the mind of Siemens also passed to the maintenance of solar fires, and occupied itself with the supply of fuel to the sun. Some physicists have attributed the continuance of solar heat to the contraction of the solar mass, and others to the impact of cometary matter. Imbued with the idea of regeneration, and seeking in nature for that thrift of power which he, as an inventor, had always aimed at, Siemens suggested a hypothesis on which the sun conserves its heat by a circulation of its fuel in space. The elements dissociated in the intense heat of the glowing orb rush into the cooler regions of space, and recombine to stream again towards the sun, where the self-same process is renewed. The hypothesis was a daring one, and evoked a great deal of discussion, to which the author replied with interest, afterwards reprinting the controversy in a volume "On the Conservation of Solar Energy." Whether true or not, and time will probably decide, the solar hypothesis of Siemens revealed its author in a new light. Hitherto he had been the ingenious inventor, the enterprising man of business, the successful engineer; but now he took a prominent place in the ranks of pure science and speculative philosophy. The remarkable breadth of his mind and the abundance of his energies were also illustrated by the active part he played in public matters connected with the progress of science. His munificent gifts in the cause of education, as much as his achievements in science, had brought him a popular reputation of the best kind; and his public utterances in

connection with smoke abatement, the electric light, electric railways, and other topics of current interest, had rapidly brought him into a foremost place among English scientific men. During the past five years, Siemens advanced from the shade of mere professional celebrity into the strong light of public fame.

Elected President of the British Association in 1882, and knighted in 1883, Siemens was a member of numerous learned societies both at home and abroad. In 1854, he became a member of the Institution of Civil Engineers; and in 1862 he was elected a Fellow of the Royal Society. He was President of the Society of Telegraph Engineers and the Institution of Mechanical, besides being a Member of Council of the Institution of Civil Engineers, and a Vice-President of the Royal Institution. The Society of Arts, as we have already seen, was the first to honour him in the country of his adoption, by awarding him a gold medal for his regenerative condenser in 1850; and last year he became its chairman. Many honours were conferred upon him in the course of his life—the Telford prize in 1853, gold medals at the various great exhibitions, including that of Paris in 1881, and a *grand prix* at the earlier Paris Exhibition of 1867 for his regenerative furnace. In 1874, he received the Royal Albert medal for his researches on heat, and in 1875 the Bessemer medal of the Iron and Steel Institute. Moreover, a few days before his death, the Council of the Institution of Civil Engineers awarded him the Howard quinquennial prize for his advances in the manufacture of iron and steel. In 1869, the University of Oxford conferred upon him the high distinction of D.C.L. (Doctor of Civil Law); and, besides being a member of several foreign societies, he was a Dignitario of the Brazilian Order of the Rose, and a Chevalier of the Legion of Honour.

Rich in honours and the appreciation of his contemporaries, in the prime of his working power and influence for good, and at the very climax of his career, Sir William Siemens was called away. The news of his death came with a shock of surprise, for hardly any one knew he had been ill. He died on the evening of Monday, November 19, 1883, at nine o'clock. A fortnight before, while returning from a managers' meeting of the Royal Institution, in

company with a friend, he tripped upon the kerbstone of the pavement, after crossing Hamilton Place, and fell heavily to the ground, with his left arm under him. Though a good deal shaken by the fall, he attended at his office in Queen Anne's Gate, Westminster, the next and for several following days; but the exertion proved too much for him, and almost for the first time in his busy life he was compelled to lay up. On his last visit to the office he was engaged most of the time in dictating to his private secretary a large portion of the address which he intended to deliver as Chairman of the Council of the Society of Arts. This was on Thursday, the 8th November, and the following Saturday he awoke early in the morning with an acute pain about the heart and a sense of coldness in the lower limbs. Hot baths and friction removed the pain, from which he did not suffer much afterwards. A slight congestion of the left lung was also relieved; and Sir William had so far recovered that he could leave his room. On Saturday, the 17th, he was to have gone for change of air to his country house at Sherwood, near Tunbridge Wells; but on Wednesday, the 14th, he appears to have caught a chill which affected his lungs, for that night he was seized with a shortness of breath and a difficulty in breathing. Though not actually confined to bed, he never left his room again. On the last day, and within four hours of his death, we are told, his two medical attendants, after consultation, spoke so hopefully of the future, that no one was prepared for the sudden end which was then so near Heart disease, either aggravated or induced by the fall, was the immediate cause; but the opinion has been expressed by one who knew him well, that Siemens "literally immolated himself on the shrine of labour." At any rate he did not spare himself, and his intense devotion to his work proved fatal.

Every day was a busy one with Siemens. His secretary was with him in his residence by nine o'clock nearly every morning, except on Sundays, assisting him in work for one society or another, the correction of proofs, or the dictation of letters giving official or scientific advice, and the preparation of lectures or patent specifications. Later on, he hurried across the Park "almost at racing speed," to his offices at Westminster, where

the business of the Landore-Siemens Steel Company and the Electrical Works of Messrs. Siemens Brothers and Company was transacted. As chairman of these large undertakings, and principal inventor of the processes and systems carried out by them, he had a hundred things to attend to in connection with them, visitors to see, and enquiries to answer. In the afternoon and evenings he was generally engaged at council meetings of the learned societies, or directory meetings of the companies in which he was interested. He was a man who took little or no leisure, and though he never appeared to over-exert himself, few men could have withstood the strain so long.

Siemens was buried on the 23rd of November, in Kensal Green Cemetery. The interment was preceded by a funeral service held in Westminster Abbey, and attended by representatives of the numerous learned societies of which he had been a conspicuous member, by many leading men in all branches of science, and also by a large body of other friends and admirers, who thus united in doing honour to his memory, and showing their sense of the loss which all classes had sustained by his death.

Siemens was above all things a "labourer." Unhasting, unresting labour was the rule of his life; and the only relaxation, not to say recreation, which he seems to have allowed himself was a change of task or the calls of sleep. This natural activity was partly due to the spur of his genius, and partly to his energetic spirit. For a man of his temperament science is always holding out new problems to solve and fresh promises of triumph. All he did only revealed more work to be done; and many a scheme lies buried in his grave.

Though Siemens was a man of varied powers, and occasionally gave himself to pure speculation in matters of science, his mind was essentially practical; and it was rather as an engineer than a discoverer that he was great. Inventions are associated with his name, not laws or new phenomena. Standing on the border-land between pure and applied science, his sympathies were yet with the latter; and as the out-going President of the British Association at Southport, in 1882, he expressed the opinion that "in the great workshop of nature there are no lines of demarkation to be

drawn between the most exalted speculation and common-place practice." The truth of this is not to be gainsaid, but it is the utterance of an engineer who judges the merit of a thing by its utility. He objected to the pursuit of science apart from its application, and held that the man of science does most for his kind who shows the world how to make use of scientific results. Such a view was natural on the part of Siemens, who was himself a living representative of the type in question; but it was not the view of such a man as Faraday or Spottiswoode, whose pure aim was to discover truth, well knowing that it would be turned to use thereafter. In Faraday's eyes the new principle was a higher boon than the appliance which was founded on it.

Tried by his own standard, however, Siemens was a conspicuous benefactor of his fellow-men; and at the time of his decease he had become our leading authority upon applied science. In electricity he was a pioneer of the new advances, and happily lived to obtain at least a Pisgah view of the great future which evidently lies before that pregnant force.

If we look for the secret of Siemens's remarkable success, we shall assuredly find it in an inventive mind, coupled with a strong commercial instinct, and supported by a physical energy which enabled him to labour long and incessantly. It is told that when a mechanical problem was brought to him for solution, he would suggest six ways of overcoming the difficulty, three of which would be impracticable, the others feasible, and one at least successful. From this we gather that his mind was fertile in expedients. The large works which he established are also a proof that, unlike most inventors, he did not lose his interest in an invention, or forsake it for another before it had been brought into the market. On the contrary, he was never satisfied with an invention until it was put in practical operation.

To the ordinary observer, Siemens did not betray any signs of the untiring energy that possessed him. His countenance was usually serene and tranquil, as that of a thinker rather than a man of action; his demeanour was cool and collected; his words few and well-chosen. In his manner, as well as in his works, there was no useless waste of power.

To the young he was kind and sympathetic, hearing, encouraging, advising; a good master, a firm friend. His very presence had a calm and orderly influence on those about him, which when he presided at a public meeting insensibly introduced a gracious tone. The diffident took heart before him, and the presumptuous were checked.

Of his domestic qualities we will not speak; but we believe that those virtues which accompanied him into public life did not desert him in private. In losing him, we lose not only a powerful intellect, but a bright example, and an amiable man.

ORIGINAL COMMUNICATIONS.

To the Secretary,

Society of Telegraph-Engineers and Electricians.

DEAR SIR,

I send you a description of a curious phenomenon observed on the passage of the "Kaisar-i-Hind" from Aden to Colombo. I have never seen anything like it in all my experience of Indian waters.

ARTHUR W. STIFFE,
Member.

CALCUTTA,
21st April, 1884.

Peninsular and Oriental steamship "Kaisar-i-Hind;" length of ship, 400 feet.

Date—18th March, 1884; local time, between 7.45 and 8.15 p.m. (*circa*).

Weather—Water smooth, slight ripple only from light easterly wind; fine evening; light clouds overhead, passing from south-east.

Position—About half way between Straits of Babelmandeb and Aden; ship heading east.

Observed strange waves or pulsations of luminosity in the sea, passing like waves from north to south, varying in deviation perhaps 20° on each side of the pole. They appeared to pass through the ship, as it were, and reappeared on the other side—that is, the ship did not appear at all to interfere with their progress. It lasted for quite half an hour.

They passed with extreme velocity; I estimate at least a mile a minute, more probably *two*. There was no real movement of matter. On looking down at the water from the ship's side, it

became from time to time luminous in patches, flashed as it were into light for a moment, and then again became dark. To look at the sea more in the distance, it appeared as if it were bands of light with dark intervals.

The moon was near the last quarter, and did not rise till about four hours afterwards.

I estimated the amplitude of a "wave," roughly, as equal to the length of our ship, say, 130 yards, and the time between the waves passing at two to three seconds, but it was difficult to make an approach to exactness.

The amount of light was that of the usual maximum phosphorescence of the sea on dark nights.

Can this be connected with electrical causes? If so, what?

CALCUTTA, 15*th June*, 1884.

DEAR MR. WEBB,

In reply to your letter drawing my attention to Professor Perry's explanation given in Vol. VIII., p. 360, of our Journal, of a somewhat similar phenomenon observed some years ago, I would mention that that explanation is quite inadmissible in my case. There was no wave motion, and the speed was immeasurably in excess of any wave movement of the water, except the tide wave, which has an enormous amplitude. Further, they were not concentric bands, but parallel approximately straight bands. Thus the length of time the phenomenon continued, the ground the ship covered in the meantime, are puzzling items. The bands had the effect of an illumination of the sun, such as, on a small scale, would be caused by the light from an electric light being turned round, or the movement of a ray of light thrown by a movable mirror, so familiar to children. I am quite at a loss for any explanation. The sea was not in any way strongly phosphorescent.

I am, &c.,

ARTHUR W. STIFFE.

195, BROADWAY, NEW YORK CITY,
March 28, 1884.

MR. F. H. WEBB,

Secretary, Society of Telegraph-Engineers and Electricians, London, England.

DEAR SIR,

In looking through Vol. XII. of the Journal of your Society, I notice, in the report of the discussion which followed the reading of Mr. A. J. S. Adams' paper on "Earth Currents," etc., the request made by Mr. C. A. Morgan, that some telegraphist in America would furnish information regarding the effect of electrical storms on the duplex or quadruplex systems in this country.

Although rather late in the day, I should like to report the effects of such storms as they came under my observation.

I have partial supervision of about twenty quadruplex sets running out of New York north-east, west, south, and north (Western Union Telegraph Company's wires).

I recollect, on the occasion of the last great electrical storm (if I mistake not, that of November 17, 1883), that we began to notice trouble from "foreign" currents as early as six a.m.; and I first remarked the approach of the storm on those multiplex "sets" which run north-easterly from New York City. Thus the clerks between Bangor, Maine, and Sydney, Cape Breton, first complained of the interference about two a.m. The storm appeared to travel westwardly and south-westwardly. I refer to the storm as a body, not to the direction of the currents, for, as we do not use galvanometers in our circuits here, we knew nothing of the direction in which they were moving. The storm grew gradually in intensity. Before it had reached its maximum intensity it seemed to act spasmodically—at one time neutralising our battery currents, and at another time surcharging the wires.

At three p.m. of that day our entire quadruplex system was rendered useless by the storm, it appearing to be at its height at that time. In consequence, business was suspended altogether

for about an hour, when the storm gradually disappeared—more quickly, however, than it had approached.

Our duplex systems and single wires were also brought to a standstill, though it was my impression at the time that the single wires suffered less than the multiplex systems during the continuance of the storm.

At the next recurrence of a storm I shall endeavour to make more careful observations and records of its effects.

Yours respectfully,

WILLIAM MAVER, Jun.

ABSTRACTS.

M. DEPREZ—ELECTRIC SYNCHRONISM OF TWO RELATIVE MOVEMENTS, AND APPLICATION TO THE CONSTRUCTION OF A NEW FORM OF COMPASS.

(*Comptes Rendus, T.* 97, *No.* 22, 1883, *p.* 1193.)

The apparatus proposed by the author consists of two parts, the so-called "annular comparative instrument" and a special form of magneto-machine. The latter has a ring of the Paccinotti type arranged between the poles of the magnet, which is free to rotate as well as the ring. Four brushes are used, arranged at right angles, and thus dividing the commutator into four quadrants; two brushes at opposite ends of the same diameter are joined up to the comparative instrument, which consists of a fixed iron ring with two windings of wire, split up in such a way that the successive sections alternately form part of one or the other winding. If two electric currents of different strength are passed through this instrument, four poles will be formed on the circumference of the ring, the lines joining which will be at right angles; but the magnetic forces exerted in their direction will combine to form a resultant, the position of which will depend on the proportion of the two currents, and not on their absolute strength. A magnetic needle suspended at the centre of the apparatus will place itself along this polar line.

If now this "annular comparative instrument" is joined up to the Paccinotti ring, and the ring is rotated while the magnet remains fixed, the same result will be attained as though the ring rotated inside two distinct magnetic fields. Now each pair of brushes will collect separately the current due to the magnetic influence of the perpendicular component at its line of contact, and the comparative instrument will be traversed by two currents of different strength, which will cause the magnet needle to take up a definite position. Suppose this the initial position, now rotate the magnet, leaving the brushes fixed; then to every angle described by the polar line will correspond a definite value of the ratio of the two components of the magnetic field, and therefore of the two currents produced. It may be concluded that if the brushes and the magnet are rotated at different rates, the comparative instrument, which may be placed anywhere, will show the relative displacement of the two.

To adapt such an apparatus for use as a compass, the axis of the rotating ring is placed vertically, and the inducing magnet is done away with, being replaced by the natural magnetic field of the earth. The resultant of the earth's magnetic force at a point can be resolved into two components, the one being a horizontal projection of the resultant, and coinciding with the magnetic

meridian—this force replaces the permanent magnet; the second is vertical to the former, and therefore without action on the ring. If, then, the brushes remain fixed, any change, however small, in the magnetic meridian will produce a corresponding change in the comparative instrument. This latter may be placed in any convenient position, while the ring may be placed at the mast-head of a ship, out of the way of all influence due to the presence of masses of iron.

E. E. BLAVIER—EARTH CURRENTS.

(*Comptes Rendus, T.* 97, *No.* 22, 1883, *p.* 1196.)

The author has made a series of researches on some of the French subterranean lines with the aid of a photographic apparatus similar to that employed by Mascart to record the variations in the earth's magnetic elements. The instruments used were the galvanometers of Deprez and d'Arsonval, and their deflections were recorded on a band of paper photographically prepared by the action of the light rays thrown from the mirrors of the instruments. As it was more important to measure the E.M.F. between the two earths of the telegraph wire than the actual strength of the currents, the total resistance of the circuit was maintained constant at 10,000 ohms by the introduction of resistance coils. As a means of control, the constant of each galvanometer was determined every morning with a Daniell cell with 20,000 ohms in a local circuit.

The experiments begun last August are still being continued, and have already led to some noteworthy results.

The direction and strength of earth currents depend solely on the difference of potential between the two points where the conductor makes earth, and are independent of its route. Thus two wires from Paris to Nancy, one overhead through Châlons and the other underground through Reims, always gave identical curves: from which it results that the secondary currents due to induction, to leakage, and to atmospheric electricity do not affect the curves. Again, it appears that, contrary to the more generally accepted opinion, underground lines are not more affected by earth currents than overhead lines; or, if they are more affected, this is rather due to their less resistance, being made of copper, and to the weaker currents and more delicate instruments with which they are worked.

The curves are also identical for two lines of equal length, which do not terminate in the same point, but follow about the same route—*e.g.*, Paris to Reims and Paris to Châlons. Where the lengths are different, Paris to Nancy and Paris to Châlons, the currents follow the same phases, but their strength varies as the distance of the extreme points, the total resistance of the line being kept constant, as already stated.

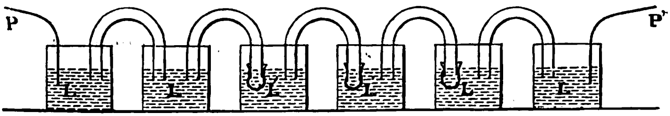
The same results are obtained in comparing the curves of two lines in diametrically opposite directions, as from Paris to Lille on the one side, and Paris to Lyons on the other. The author therefore thinks that it might be quite possible to study earth currents on a series of lines of short lengths radiating from a central station.

The currents vary continually in direction and strength; but it is as yet very difficult to determine the laws governing them, especially on lines running east and west. On north and south lines the photographic curves show that from 9 a.m. to noon the earth current flows from north to south, and attains its maximum strength at 10.30 a.m.

E. BICHAT and R. BLONDLOT—MEASUREMENT OF THE DIFFERENCE OF POTENTIAL OF THE ELECTRIC LAYERS COVERING TWO LIQUIDS IN CONTACT.

(*Comptes Rendus, T.* 97, *Nos.* 22 *and* 23, 1883, *pp.* 1202 *and* 1293.)

Suppose two vessels, X and Y, each filled with same liquid, L, and united by a siphon also filled with the same liquid. Into each vessel dips a platinum electrode, P_1 and P_2, these being connected up to opposite pairs of quadrants of a Mascart electrometer; the needle will come to rest at some point which will not be the zero, owing to the inequality of the platinum electrodes, but which is taken as the zero of the experiments. Now remove the siphon, and between X and Y place a third vessel, Z, containing a liquid, L′. Connect X and Z by a siphon filled with the liquid L, and having its end in the vessel Z, covered by a parchment membrane. By a method which is an adaptation of W. Thomson's water-dropping apparatus for measuring atmospheric



1 2 3 4 5 6

electricity, and is fully described by the authors, the potentials of the layers of air above the two liquids are made equal; and it will then be found that the difference of potential between P_1 in X and P_2 in Y is not the same as in the first experiment. It will be increased by the differences $\frac{L}{L'} + \frac{L'}{air} \div \frac{air}{L}$, which is equal to the difference of potential of the two liquids $\frac{L}{L'}$. By introducing a known E.M.F. into the circuit between P_1 and the electrometer, and regulating it, we can just balance the difference of potential $\frac{L}{L'}$, and can therefore measure it.

In the first series of experiments the authors found that if L and L′ were the same liquid, no difference of potential was observed, and that the water-dropping apparatus had no effect on the values obtained, whichever liquid was allowed to issue in drops.

In their second communication to the Académie des Sciences, the authors show how their method may be extended to the measurement of the differences of potential of several liquids in contact, by comparing each of them with the same liquid L.

Suppose six vessels, as shown in the figure; then, measuring the difference of potential between P and P′, we have

$$E = L \mid P + L' \mid L + L'' \mid L' + L \mid L'' + P' \mid L.$$

Now interchange the vessels 1 and 6, then

$$E' = L \mid P' + L' \mid L + L'' \mid L' + L \mid L'' + P \mid L.$$

Whence

$$L'' \mid L' = \frac{E + E'}{2} + L \mid L' - L \mid L'',$$

and as $L \mid L'$ and $L \mid L''$ are known, we can determine $L'' \mid L'$.

The following table shows the results obtained :—

First Liquid.	Second Liquid.	Volts.
HNO_3 (commercial)	$9\,H_2O + 1\,H_2SO_4$	— 0·545
$9\,H_2O + 1\,H_2SO_4$	Na_2SO_4	— 0·148
HNO_3 (commercial)	Do.	— 0·677
HCl (pure)	Do.	— 0·575
$5\,H_2O + 1\,KHO$	Do.	+ 0·052
$5\,H_2O + 3\,KHO$	Do.	+ 0·154
$9\,H_2O + 3\,ZnSO_4$	Do.	Nil.
$CuSO_4$ (saturated)	Do.	Nil.
$9\,H_2O + 3\,ZnSO_4$	$CuSO_4$ (saturated)	Nil.

A. CHARPENTIER.—PERCEPTION OF DIFFERENCES OF BRILLIANCY.

(*Comptes Rendus, T.* 97, *No.* 24, 1883, *p.* 1373.)

EFFECTS OF DIFFERENCES OF COLOUR.

(*Idem, No.* 25, *p.* 1431.)

The instrument used in these researches was the differential photometer. A translucent screen is placed at the bottom of a dark box, and can be illuminated simultaneously both in front and behind by means of two independent light regulators. One of these regulators is placed on one side with respect to the axis of the instrument, and sends its light on to the front face of the screen after reflection from a glass plate placed at an angle of 45° in front of the screen, which is seen through the glass by the observer in front. The light behind is stopped off by draphragms and only illuminates part of the screen. It is then possible to determine exactly the amount of supplementary light necessary to enable this small surface to be distinguished from the generally illuminated screen.

The author finds that the law, that the *ratio* of the supplementary light to the main light is constant whatever their absolute values, is quite incorrect. The influence of size is considerable, thus with small objects, where the angle of vision was less than half a degree, the above ratio (differential fraction) seemed to vary inversely as the diameter of the object. The differential fraction is also inversely proportional to the square foot of the brightness of

the background. By going to extremes, taking a very small object and very feeble light, it was not possible to distinguish a point ten times brighter than the background.

The above experiments were made with white objects. The author made a further series with coloured objects, the amount of light used being just sufficient to render the object visible after the eye had been in the dark for twenty minutes. On comparing the colours of the spectrum it was found that the differential fraction increases with the refrangibility of the colours. In this case also the differential fraction always varies inversely as the square foot of the illumination.

In conclusion the author enunciates four laws:—

For similar luminous intensities and for the same object, the differential sensibility is closely related to the wave length.

The differential sensibility is more delicate, *i.e.*, the differential fraction is smaller for the less refrangible colours.

For equal illumination we therefore distinguish best the forms of objects of the less refrangible colours.

The differential sensibility of the light of a Carcel lamp lies between that of yellow and green.

J. MACÉ DE LÉPINAY—PRACTICAL METHOD OF COMPARING DIFFERENTLY COLOURED LIGHTS.

(*Comptes Rendus, T.* 97, *No.* 25, 1883, *p.* 1428.)

The author starts from the law laid down by Becquerel, that when bodies at the same temperature, and with different powers of emission, are placed in a dark enclosure, they emit light of very different intensity, but of the same composition.

If I is the intensity of one source of light compared with a standard, R the intensity of a red ray of a fixed wave length, G the intensity for a green ray, then, on substituting another source at the same temperature, the three values I, R, and G will remain proportional, and for any sources whatever the ratios $\frac{I}{R}$ and $\frac{G}{R}$ will remain constant. On the other hand, if the temperature is made to vary continuously, these ratios will vary continuously; and we may conclude that

$$\frac{I}{R} = F\left(\frac{G}{R}\right)$$

If by a sufficient number of experiments we can determine the nature of this function, we can determine I by measuring R and G.

In the above form, however, it would be necessary to employ a spectro-photometer, which is a very delicate instrument. For practical purposes, however, we can make use of a Foucault photometer, by comparing the shadows through two solutions, one red and one green, if (1) they are always of the same thickness and the same strength, and (2) if they furnish radiations which are sensibly simple, so that the two surfaces compared have the same tint.

The author has found the following the best solutions:—for the red, a solution of pure perchloride of iron in water, of strength 38° Baumé (specific gravity = 1·3574); for the green, a solution of pure chloride of nickel in water, of strength 18° Baumé (specific gravity = 1·1425).

The following is the formula obtained by the author by the method of least squares from a series of 52 observations:—

$$\frac{R}{I} - 1 = 0{\cdot}208\left(1 - \frac{G}{R}\right)$$

E. DUCRETET—CALIBRATION OF GALVANOMETERS.

(Comptes Rendus, T. 97, No. 26, 1883, p. 1477.)

The method is applicable to those galvanometers in which the magnetic field is produced by powerful permanent (?) magnets; and consists in coiling insulated wire round the magnet. Every time that the calibration has to be verified, the magnet is brought back to its original state of saturation by sending a current from several large Daniell cells (the round shape, 0·22 mètres high is very suitable) through the coils.

M. IZARN—REPULSION OF TWO CONSECUTIVE PORTIONS OF THE SAME CURRENT.

(Comptes Rendus, T. 98, No. 3, 1884, p. 143.)

It has generally been considered that Ampère's classic experiment should not be unhesitatingly accepted as proof of the above, as the action of the wire connecting the two horizontal pieces floating on the mercury must affect the result. If, however, the floating portion of the apparatus be turned round so that the current traversing the mercury is obliged to turn back in order to traverse the horizontal portion, these would be attracted, the action on the other remaining the same as before. This actually takes place when the current traversing the horizontal part is from east to west, the mercury bath lying in a north and south plane; but for the opposite direction there is repulsion. The experiment is therefore complicated by the action of the earth's magnetism. This may be eliminated by using an astatic system, *i.e.*, two mercury baths, with two floating wires joined together, so that the current circulates through them in opposite directions. The author specially warns intending experimenters to make use of mercury which is perfectly clean. It should be poured into the bath from a funnel just before the experiment, care being taken not to allow the last portion, containing the oxidised surface, to run through.

F. LUCAS—THEORY AND FORMULÆ OF ALTERNATING CURRENT MAGNETO MACHINES.

(Comptes Rendus, T. 98, No. 11, March 17, 1884, p. 670.)

In this type of machine the armature consists of a number of similar bobbins, $N = \mu\,\nu$, where the factor μ represents the number of bobbins con-

nected in series, and ν the number connected parallel. If now the armature make n revolutions per minute, the results will vary according to the resistance, R, in the outer circuit. Let R be measured in ohms, as well as r, the resistance of the armature, I in ampères, and T in horse-power (French). By the law of conservation of energy we have a first equation

$$(R + r) I^2 = 75 g T \quad \dots \quad \dots \quad \dots \quad \dots \quad (1)$$

A second equation may be obtained from experiment. Allow the machine to work through an unknown external resistance, but measure I and T, subtracting from this latter the work lost in transmission and friction. Making a great number of experiments with varying R, we obtain a curve showing the relation between I and T, and the author has found that the law of this relation is represented by a parabola of the second order, passing through the origin of co-ordinates, and with a vertical axis. Hence

$$\rho (I - h)^2 = 75 g (k - T) \quad \dots \quad \dots \quad \dots \quad (2)$$

The numerical values of the three parameters ρ (resistance), h (current), and k (work) are obtained from the curve, as this passes through the origin

$$\rho h^2 = 75 g k \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

Eliminating T between equations (1) and (2) and putting

$$\phi = 2 \rho h \quad \dots \quad \dots \quad \dots \quad \dots \quad (4)$$

we arrive at

$$(R + r + \rho) I = \phi \quad \dots \quad \dots \quad \dots \quad \dots \quad (5)$$

The parameter ϕ is expressed in volts, as an E.M.F.; ρ represents a *fictitious* resistance, to which no consumption of mechanical work corresponds.

To determine ϕ and ρ directly, two known values, R′ and R″, are given to R, and the corresponding currents, I′ and I″, are measured.

$$\left.\begin{aligned} \rho &= \frac{R'' I'' - R' I'}{I' - I''} - r \\ \phi &= (R'' - R') \frac{I' I''}{I' - I''} \end{aligned}\right\} \quad \dots \quad \dots \quad \dots \quad (6)$$

So far the speed has been assumed constant. If now we make n vary, the parameters ρ and ϕ increase in direct proportion to n. Hence

$$\left.\begin{aligned} \phi &= a + \alpha n \\ \rho &= b + \beta n \end{aligned}\right\} \quad \dots \quad \dots \quad \dots \quad \dots \quad (7)$$

and equation (5) becomes

$$I = \frac{a + \alpha n}{R + r + b + \beta n} \quad \dots \quad \dots \quad \dots \quad (8)$$

which the author has verified by a great number of experiments.

The coupling up in series of μ similar systems multiplies by μ the parameters ρ, ϕ, and r. The coupling up in parallel arc of ν similar systems divides by ν the parameters ρ and r, but does not alter ϕ.

If we denote by a_1, α_1, b_1, β_1, and r_1, corresponding parameters for a single bobbin, we have

$$a = \frac{\mu}{\nu} a_1 \,; \; \alpha = \frac{\mu}{\nu} \alpha_1 \,; \; r = \frac{\mu}{\nu} r_1 \,; \; b = \mu b_1 \,; \; \beta = \mu \beta_1 \quad \dots \quad (9)$$

and the electrical work in the external circuit is given by the equation

$$75\, g\, T_e = \frac{N\, (a_1 + \alpha_1 n)^2\, R}{[\nu R + \mu\, (r_1 + b_1 + \beta_1 n)]^2} \quad \ldots \quad \ldots (10)$$

This becomes a maximum when

$$\nu R = \mu\, (r_1 + b_1 + \beta_1 n) \quad \ldots \quad \ldots \quad \ldots (11)$$

and its value is then

$$75\, g\, T'_e = \frac{N\, (a_1 + \alpha_1 n)^2}{4\, (r_1 + b_1 + \beta_1 n)}.$$

G. CHAPERON—A PROBABLE CAUSE OF THE DISAGREEMENT BETWEEN THE ELECTRO-MOTIVE FORCE OF BATTERIES AND THE ELECTRO-CHEMICAL DATA.

(Comptes Rendus, T. 98, No. 12, March 24, 1884, p. 729.)

One of the most marked cases is in the instance of a cell formed of aluminium in dilute sulphuric acid, and copper in saturated copper sulphate solution. Theoretically, this combination ought to give an E.M.F. of 2·06 volts; measured on an electrometer the E.M.F. is only 0·62 volt.

The author, thinking that this might be traced to polarisation due to the inherent properties of the metals, even when no current was passing, undertook the following experiment:—By means of a suitable key the two poles of the battery were disconnected and immediately joined up to the two electrodes of a one-microfarad condenser. With non-polarising cell, such as distilled zinc in sulphate of zinc and a source of one volt, in three taps of the key the condenser will be charged to some hundredths of a volt, and this charge cannot be increased appreciably by further connection to the battery; but with a polarising cell three or four taps of the key are sufficient to charge the condenser to a potential of very nearly one volt.

A study of the curves plotted from the results obtained shows that the metals experimented upon give rise to polarisable systems up to the decomposition of the electrolyte, the difference of potential retained by the electrodes increasing, in accordance with a continuous law, from zero to the point of decomposition, and even beyond it.

Let T_{mR} and T_{mH} be the electrical energy corresponding for a given temperature and pressure to the absorption or restitution of the elements R and H of an electrolyte by an electrode of a metal, *m*, which becomes polarised. The chemical work corresponding to the electrical energy given up by two polarisable electrodes will be generally

$$T_{RH} - T_{mR} - T_{mH},$$

where T_{RH} is the energy of formation of the electrolyte. If the polarisation follows a continuous law, this value of the chemical energy ought to vary from T_{RH} to zero between the point of decomposition and the state of neutrality of the electrodes, which is then characterised by the equation

$$T_{RH} - T_{mR} - T_{mH} = 0.$$

In this interval, T_{mR} and T_{mH} will vary from zero to two positive values.

Each of these quantities, then, at its limiting value will be less than T_{RH}. It is the limiting value of T_{mR} satisfying the above equation which should be used in the calculation of the theoretical E.M.F., instead of the heat of combination.

A. CALLAUD—NOTE ON A MODIFICATION OF LIGHTNING-ROD CONDUCTORS.

(*Comptes Rendus, T.* 98, *No.* 13, *March* 31, 1884, *p.* 782.)

The conductors, as hitherto made by the author for the War Department, have been buried in trenches excavated in the ground, being first surrounded with coke. Free access is allowed to the moisture, so as to increase the conductivity. This arrangement, which is a good one so long as the conductor remains intact, becomes defective if oxidation sets in. To remedy this it is proposed to cover each wire of the conductor with hemp saturated with carbonate or oxide of lead. The wires are then made up into a five-strand cable, an unprotected wire being placed in the centre. The whole cable is then covered with tape, also saturated with carbonate or oxide of lead.

E. REYNIER—ELECTRO-MOTIVE FORCE OF DANIELL CELL.

(*L'Electricien, T.* 7, *No.* 70, *March* 1, 1884, *p.* 201.)

The results obtained by the author are given in the accompanying table, the E.M.F. being in volts:—

Copper Compartment.	Zinc Compartment.	Amalgamated Zinc.	Ordinary Zinc.
Solution of	Solution of		
Sul. of copper, saturated	Sul. of zinc	1·079	1·068
" " "	" " acidified ...	1·103	1·06
" " acidified	" "	1·03	1·025
" " "	" " acidified ...	1·066	1·03
" " saturated	Sea salt	1·145	1·14
" " acidified	"	1·115	1·09
" " "	" acidified	1·125	1·09
" " saturated	Dilute sulphuric acid ...	1·134	1·05
" " acidified	" " ...	1·119	1·027
" " saturated	Partition	1·10	1·04
" " acidified	"	1·05	1·00

The sulphate of zinc solution was made by dissolving 500 parts by weight in 1,000 parts of water; the solution of sea salt, of 200 parts in 1,000. Where the word *acidified* is added, $\frac{1}{20}$ by volume of sulphuric acid was added. In the two last lines of the table, the zinc was wrapped in an envelope of parchment paper and plunged directly into the copper solution.

R. ARNOUX—RAPID METHOD OF MEASUREMENT OF LARGE DIFFERENCES OF POTENTIAL.

(*L'Electricien, T. 7, No. 72, April* 1, 1884, *p.* 301.)

This method is not new, being based on the use of a condenser, but is nöteworthy on account of the rapidity and accuracy with which it can be carried out. The instrument used is the dead-beat galvanometer of Deprez and D'Arsonval (see Abstract in Journal, No. 49, p. 448). The theory of the method is the following:—If there be two condensers, of capacities C and C′, connected to two sources of E.M.F., E and E′, then we know that

$$\frac{Q}{Q'} = \frac{C}{C'} \times \frac{E}{E'}.$$

and hence

$$E = E' \times \frac{C'}{C} \times \frac{Q}{Q'} \quad \ldots \quad \ldots \quad \ldots \quad (1)$$

With a box of standard condensers, C and C′ are known, and it is only necessary to determine the ratio $\frac{Q}{Q'}$. The galvanometer mentioned is placed between the source of E.M.F. and the condenser, and the quantity of electricity which charges the condenser must pass through the swinging coil. The latter, having a small resistance but a great moment of inertia, is not displaced appreciably *during* the passing of the current. Still this variable current gives to the coil an impulse variable also at each instant, but always proportional to the strength of the current. The total impulse in the time t is therefore proportional to $\int_0^t i\, dt$; hence $\int_0^t P\, dt = K \int_0^t i\, dt$.

But $i = \frac{dQ}{dt}$; since $dQ = i\ dt + t\ di$, in which the second term may be neglected, t being very small. Hence

$$\int_0^t P\ dt = K \int_0^t \frac{dQ}{dt}\ dt = K \times Q.$$

The impulses given to the coil are hence proportional to the quantities of electricity multiplied by a constant.

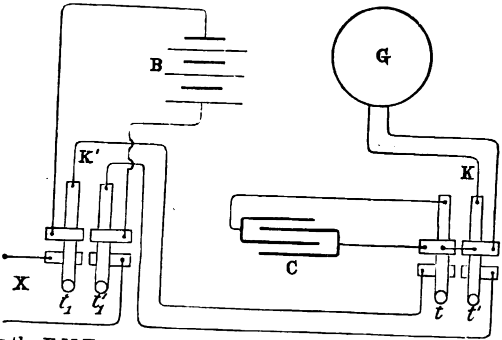
It is also shown that the velocities of displacement of the coil are proportional to the arcs of torsion, and consequently the whole movements are as the amplitudes of the first oscillations. We may therefore in equation (1) substitute these arcs for Q and Q_1; hence

$$E = E' \times \frac{c'}{c} \times \frac{\phi}{\phi'} \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

The author took care to ascertain that (1st) the torsional couples were strictly proportional to the deflections, and (2nd) the deflections proportional to the charges. The tables of results given sufficiently prove the accuracy of the method in these two points. They further show that, by charging a condenser of one microfarad to a potential of one volt, a deflection of 17·47 divisions would be obtained, so that with a $\frac{1}{1000}$ microfarad condenser a difference of

potential of 10,000 volts could be measured. Using a Holz machine and still smaller condenser (0·000197 microfarad), the author has recorded potentials of 27,000 and 33,000 volts.

The subjoined figure shows a practical way of arranging the apparatus, in



which X is the E.M.F. to be measured. The key K' is only used to verify the constant of the galvanometer. The actual measurement is made thus:—On pressing down the two keys, *t* and *t'*, of K, the current charging the condenser from the battery, B, passes through G, and causes a deflection which is read off. On releasing the keys, the galvanometer is short-circuited and returns immediately to zero, and the condenser is discharged. Now depress the key K' and then K, and the condenser is charged from the unknown source of E.M.F. instead of from the battery.

The special merit claimed by the author for his system is that it does not consume any of the energy in the circuit, the E.M.F. of which is to be measured, and he adds that in the experiments at the Northern of France Railway about $\frac{1}{45}$ of the energy transmitted was used up in measuring instruments, and $\frac{1}{15}$ in the experiments at Munich.

F. LARROQUE—EARTH CURRENTS.

(*La Lumière Electrique, Vol. XI., No. 7, Feb.* 16, 1884, *p.* 327.)

If we suppose for a moment that there is only one earth current, then, if the earth is of the nature of a solenoid, any conductor inserted between two points would form a by-pass and be traversed by a current; or we may suppose that the earth current is propagated as a sheet, and that it has as its point of origin half a great circle of the sphere, and that its path is marked out by a system of parallels embracing the whole surface; we may then imagine the zone comprised between two adjacent parallels as a cylindrical surface. Were the earth a perfectly homogeneous conductor, no current would flow in a by-pass circuit which was parallel to the generatrix of the cylinder; but the earth is by no means a homogeneous conductor, both on account of its geological formation and the variations in temperature of different parts.

The experiments of the author were made with short insulated conductors, some eight or ten mètres long only, attached to plates buried to a depth of one mètre in the clay soil. In 1882, three such lines were set up, each with its highly sensitive galvanometer, the lines being placed N. to S., E. to W., N.W. to S.E. The results obtained were not very satisfactory, but at least it was evident that the currents on the N. to S. wire were much more irregular than those on the other two wires, and that oscillations in the currents occurred between 7 and 11 a.m. and between 4 and 10 p.m.

In the following year, 1883, the author made a different disposition. He had two wires, A about eight mètres long, B about ten mètres long, which were placed parallel to each other; the earth plates were set at right angles to the wires, and the plates of B, the longer wire, enclosed those of A, the shorter wire. Each circuit was made equal in resistance, and comprised a very delicate galvanometer. By means of two contact keys the circuit B could be interrupted at both ends. The system of two wires was successively placed so as to coincide with all points of the compass, and by making and breaking contact in B the author sought to find out the direction taken by the earth currents. If they were parallel to the wires, then, on closing the B circuit, the deflection of the galvanometer in the A circuit would diminish until it was equal to that in the B circuit. If, on the other hand, the direction of the currents was at right angles to that of the wires, the closing of the B circuit would have little or no effect on the A galvanometer.

From a series of observations extending over rather more than nine weeks, the author concludes that there is only one earth current, that its direction varies between W.N.W. to E.S.E. and W. to E.; sudden and peculiar deflections correspond to magnetic disturbances; accidental currents of short duration and unknown origin show themselves at rare and irregular intervals in a direction perpendicular to the earth current.

A. MINET—USE OF THE CALORIMETER FOR THE EXACT MEASUREMENT OF CURRENTS.

(*La Lumière Electrique, Vol. XII., pp.* 122-207.)

In a series of articles, not yet completed, the author investigates very completely the adaptation of the calorimeter for the measurement of currents. The apparatus is not intended for every-day use, as its manipulation requires considerable care; but for exact laboratory determinations, and for the calibration of other instruments, it may be of valuable service.

The form recommended by the author is the mercury calorimeter of Favre, with a long horizontal graduated glass tube in which the mercury column expands. The unit of heat used is the lesser calorie—*i.e.*, the quantity of heat necessary to raise one gramme of water one degree centigrade, and which is equivalent to 0·424 kilogrammètres. The dimensions of the calorimeter used by the author were so arranged that this amount of heat caused an elongation of the mercury column of 0·14 mm.

The whole calculation rests of course on Joule's law,

$$H = \frac{R\,I^2}{g},$$

where H is the number of kilogrammètres of energy expended as heat per second, and the quantity of heat which will be indicated by the calorimeter when the experiment is made during some time will be

$$q = \frac{R\,I^2\,t}{g \times 0\cdot424} = \frac{R\,I^2\,t}{4\cdot159}.$$

If, therefore, we know either R or I, we can measure q and t, and thus arrive at I or R.

If we arrange the scale so that 1 mm. corresponds to 1 degree, then the number of degrees will be equal to $q \times 0\cdot14 = n$ (say). Thus

$$n = \frac{R\,I^2\,t}{4\cdot159} \times 0\cdot14,$$

or $I = K\sqrt{n}$ if we keep R and t constant—that is, the current will be found by multiplying the square root of the reading by a constant, as is the case with a Siemens electro-dynamometer. In practice, it is essential that the current should be kept constant, and for this purpose a galvanometer should be joined up in the circuit. This method is especially applicable for the calibration of an electro-dynamometer, for we have in the one case

$$q = K\,I^2;\ \text{in the other,}\ n = K^1\,I^2$$

hence K^1, the constant of the electro-dynamometer, is equal to $\frac{n}{q}$ K. This latter constant, K, of the calorimeter is determined once for all by the usual method—*i.e.*, the water value of the instrument is found.

The author enters at length into the formulæ for the determination of E.M.F., but it is clear how they may be arrived at.

W. HITTORF—CONDUCTIVITY OF GASES.

(*Annalen der Physik und Chemie, B. XXI., H. 1, No. 1, 1884, pp.* 90-139.)

In a very long article, of which it is not possible to give, within the limits of an abstract, more than the briefest outline, the author gives a further series of experiments, minutely described in detail, on the above phenomenon. The article has three divisions—VI. Further peculiarities of the glow in gases under small pressure. VII. Behaviour of the gaseous envelope surrounding the cathode at greater pressures. VIII. Consequences of the relations of the electrical gaseous currents so far as known at present. If the cathode with its envelope is raised to a white heat, while the anode and the rest of the gaseous particles remain cold, very few Bunsen cells are sufficient to maintain continuous currents in the highly-rarefied gases. These three sections do not conclude the subject, as a further communication is promised. The experiments were made with various gases—nitrogen, hydrogen, carbonic oxide—and air, the sizes of the tubes used and the pressures varying within wide ranges.

A. N. EMO—RESISTANCE OF VIBRATING WIRES.

(*Beiblätter*, B. 7, No. 12, *Dec.*, 1883, *p.* 907; *Riv. Sc. Industr.*, 15, *p.* 211, 1883.)

The resistances of wires stretched on a monochord were compared by means of a Wheatstone balance with a Siemens mercury unit, to which they were nearly equal. A negative result was obtained when the stretched wires were struck with a hammer, as in a pianoforte, the resistance remaining constant within the limit of 0·0007 of an ohm, whatever kind of wire was used, iron, steel, brass, or German silver.

R. LENZ—RESISTANCE OF MERCURY PURIFIED IN VARIOUS WAYS.

(*Beiblätter*, *B.* 8, *No.* 1, *Jan.*, 1884, *p.* 39.)

The varying values obtained by different experimenters in the determination of the ohm in absolute measure has led the author to investigate more closely the effect of slight impurities on the resistance of mercury. Two tubes were used, one of which was filled successively with the several samples of mercury, while the other remained unchanged; the measurements were made with a Wheatstone balance, the two tubes being placed alternately in the comparison branch. A dead-beat Siemens galvanometer of about 6,000 S.U. resistance was used. The tube and a rheostat were placed in one branch of the bridge, a copper wire in the second, and two resistances (each 10 S.U.) in the two other branches. The tubes had a section of 0·7 sq. mm. and a resistance of about 13·5 S.U.; their ends were fitted into tubulures on the sides of two glass flasks, in which hung a copper rod covered with a strip of platinum on the side next the tubulure. The tubes, as well as the copper wire, were placed in snow.

The tubes were filled by pouring mercury into one flask till above the tubulure, and into the other up to the tubulure, and then exhausting the air in the former so that the mercury flowed through the tube and filled it. The tubes were cleaned with a mixture of bichromate of potash and sulphuric acid, with nitric acid, with ammonia, with water, and finally with alcohol, and then dried by passing through them dry air filtered through cotton wool. It was found that in whatever way the mercury might have been purified, its resistance only varied about 0·01 per cent. so long as it was carefully cleaned and *freed from air*. Thirteen different methods of purification were employed, which it may be interesting to enumerate. 1. Treated with HNO_3, washed with alcohol, boiled in a vacuum. 2. The same without the boiling. 3. Distillation in Weinhold's apparatus. 4. Heated in an open dish, till a film of oxide formed, and filtered through a pin-hole in paper. 5. Digested with $KHSO_4$ for two months, washed with water and alcohol, and distilled in a vacuum. 6. The same, and then slightly heated in vacuum over sulphuric acid. 7. The remainder from the distillation of 5. 8. Treated with chloride of iron, washed and distilled in a vacuum. 9. Remainder from 8. 10. Distilled in Weinhold's apparatus, converted into nitrate, dissolved in dilute nitric acid, precipitated by electrolysis, washed in

water and in alcohol, dried over sulphuric acid in a vacuum in which it was kept. 11. As in 10, then treated as in 4. 12. Heated for three hours with strong sulphuric acid with which a few drops of nitric acid were mixed. 13. As in 3, distilled directly into the experimental tube.

No. 12 was the only sample in which any increase of resistance was observed, and it will be noted that this is the only case in which the air was not exhausted. On the other hand, in No. 13, where the removal of air was most complete the resistance was least; the difference between 12 and 13 being about 0·05 per cent. The specific gravity of the several samples lay between the extreme values 13·59869 (No. 2.) and 13·59810 (No. 9). The resistance of mercury containing oxide is the same as of the absolutely pure metal. The admixture of 0·01 per cent. of lead, followed by three successive filtrations, left the resistance unaltered, but brought down the specific gravity to 13·59783.

A. RICCO—NEW FORM OF ELECTRO-MAGNET.

(*Beiblätter, B.* 8, *St.* 4, *No.* 4, 1884, *p.* 318.)

Round an iron core, but insulated from it by a layer of paper, is wrapped a long thin piece of sheet iron. The outer end of the sheet is connected to one terminal; the inner one, which is soldered to the core, to the other one.

The attraction increases nearly proportionally to the number of convolutions from the circumference to the centre, which is also shown by the way in which iron filings heap themselves up over the centre. The carrying power is considerable, and is still further increased by applying an iron disc to the end.

If the magnet is also wound with a coil of copper wire, and the current passed only through this wire, the magnetism increases from the centre towards the circumference. If the current is made to pass through both copper wire and iron sheet in the same direction, the effects are superadded; if in opposite directions, opposite polarities are obtained at the circumference and centre.

If two such magnets are placed on an iron plate so as to form a horse-shoe magnet, this possesses very considerable carrying power, if the current passes only through the copper wire, or through copper wire and iron sheet in the same direction. By making the four polar ends convex, the harmful action of the opposite polarities of the exterior convolutions of the sheet iron is diminished.

Dr. E. BOETTCHER—AUTOMATIC CURRENT REGULATOR.

(*Centralblatt für Elektrotechnik, B.* 5, *p.* 637.)

The current to be regulated passes round a solenoid placed vertically, in the centre of which is an iron core suspended by a spring and projecting below the solenoid. To the bottom of the iron core is attached by a rod or cord a zinc plate, which dips into a vessel filled with a solution of sulphate of zinc, in which is another fixed zinc plate. If the current in the circuit diminishes, the iron core will descend, a greater surface of the zinc plate will be immersed, the resistance thereby diminished, and the current brought back

to something near its former value. With an increase of current the reverse takes place. The author appends an example, in which he works out the exact size to be given to the zinc plates, and the details of the rest of the apparatus, so that a current of 14·5 ampères supplying twenty Edison lamps should not vary more than five per cent.

F. KOHLRAUSCH—ON THE CONSTANTS OF MAGNETS.

(*Centralblatt für Elektrotechnik, B.* 6, *No.* 8, 1884, *p.* 182.)

1. *Distance apart of the Poles of a Magnet.*—For most measurements it is unnecessary to know the exact distribution of magnetism in a magnet, but it is sufficient to know the distance apart of the two poles—*i.e.*, of those two points at which the free magnetism may be assumed to act at a distance so soon as the fourth power of the ratio of the length of the magnet to the distance from the magnet approaches nearly to unity. A long series of experiments were made in several ways—by the deflection of a very short needle in the usual way by the magnet; by the simultaneous action of the magnet on two magnetometers, between which it was placed; by simultaneous action of currents in concentric circles of various diameters on the magnet.

The author proposes the term "modulus of the polar distance" for the factor K, by which the geometrical length of the bar must be multiplied in order to obtain its magnetic length—*i.e.*, the distance apart of its poles. In all, fourteen magnets were used, some being round and others square solid bars, some hollow tubes; and the mean value of K was found to be 0·83, the extreme values for different bars being 0·81 and 0·86. In other words, the pole of a magnet, so far as its action at a distance is concerned, seems to be $\frac{1}{12}$ of the total length of the bar from the end. For a circular disc the polar distance was found to be 0·8 times the diameter, and for a ring magnet 0·88 times the outside diameter.

2. *The Increase and Decrease of Magnetism by Small Forces.*—The question to be investigated is whether the increase and the decrease would be equal in quantity. Lamont has concluded that a weakening of the magnetism is more readily brought about than an increase in the ratio of about 4 : 3; the author thinks that this is not the case, but that increase and decrease are equal. He determined the specific inductive coefficients of a large number of magnets—*i.e.*, the increase and diminution of the magnetic moment of unit mass (1 gr.), which is induced by the unit of magnetising or of demagnetising force ($g^{\frac{1}{2}}\ c^{-\frac{1}{2}}$ sec.$^{-1}$). A reference to the tabulated results shows at once that the two coefficients are the same in value, any very slight differences being due to personal error in the observations.

3. *Determination of the Temperature Coefficient.*—The magnet is placed horizontally in the same plane with a needle attached to a small mirror, the centre being in the meridian of the needle, in such a position that the needle takes an east-west direction; in this position the magnet makes an angle ϕ

with the meridian. If the magnetism of the bar is altered by ΔM, the position of the needle will be altered by $\Delta\Sigma$; and then

$$\frac{\Delta M}{M} = \tfrac{1}{2} \tan. \phi \, \Delta \Sigma.$$

Let A be the distance from mirror to scale, t and t' the two temperatures, e_1 and e_2, e'_1 and e'_2, the position of the needle at the two temperatures if the bar is turned through 2ϕ, then

$$\frac{\Delta M}{M} = \frac{\tan. \phi}{8 A} \left\{ (e_1 - e_2) - (e'_1 - e'_2) \right\}$$

$$\text{And } \theta = \frac{\tan. \phi}{8 A} \left\{ \frac{(e_1 - e_2) - (e'_1 - e'_2)}{t - t'} \right\}$$

If the magnet is near the needle, a correction for length must be made, and the above equation has to be multiplied by

$$1 + \tfrac{1}{8} \frac{\lambda^2}{a^2} (3 + 5 \cos.^2 \phi)$$

where a is the distance from centre of magnet to needle, λ the polar distance of the magnet.

4. *Determination of Inertia by the Bifilar.*—The directive force, D, of the bifilar is determined from the distance apart of the threads, their length, weight, and elasticity, and the weight of the cradle, as has been already explained by the author in the *Annalen der Physik und Chemie*, T. XVII., pp. 744, 754. Knowing then the time of oscillation t, we have $K = \frac{D t^2}{\pi^2}$ for the cradle and suspension; repeating the determination of t when the body is laid in the cradle, we have K for both. If the body is magnetic, the earth's influence may be got rid of by placing the magnet east and west. Or the magnetic body may be placed in the meridian and t_1 measured when in one position, and t_2 when the magnet is turned end for end, then the time of oscillation of a non-magnetic body would be

$$\sqrt{2} \frac{t_1 t_2}{\sqrt{t_1^2 + t_2^2}}$$

W. HOLTZ—INFLUENCE OF THE SUN ON THE EARTH'S ATMOSPHERIC ELECTRICITY.

(*Centralblatt für Elektrotechnik, B. 6, No. 11, 1884, p. 256.*)

Already, in 1877, in describing a new form of his influence machine, the author had suggested that the electricity of our atmosphere might be due to the action at a distance of the sun. Since then Siemens has put forward a hypothesis according to which the sun loses matter charged with negative electricity, and is therefore positively charged. The sun thus acts on the earth in the same way as an insulated sphere charged with positive electricity would act on a smaller uninsulated sphere in its neighbourhood.

To this hypothesis the author offers some objections. It is difficult to imagine that the earth is uninsulated, and that it becomes negatively charged from the repulsion into the upper layers of the atmosphere of its positive

electricity by the positive charge of the sun, and it seems more rational to suppose that the earth is insulated. If we conceive that the earth can be charged by induction from the sun, this charge will not be on the earth's surface, but must reside on the outside of the atmospheric envelope, where the potential would be equal at all points of the imaginary sphere. This atmospheric charge cannot have any effect on the earth, just as an electrified glass globe has no effect on an electroscope inside it. But such an equal distribution of electricity cannot take place, being interfered with by the air currents and vapour in the air; and thus, the distribution not being equal, the atmospheric charge at different places might have an inductive influence on the earth itself.

If, however, while still supposing that the earth is insulated, we cannot conceive that it possesses a decided charge of electricity of one or the other name, then it may be supposed that by the inductive action of the sun the negative electricity will be attracted to that half of the earth's sphere which is turned towards the positive sun, and the positive repelled. Thus, wherever there is daylight there is negative electricity, while wherever there is night there is positive. If this be so, while the distribution of electricity remains fixed as regarded from the sun, owing to the earth's rotation every part of it must be alternately negative and positive in the course of each twenty-four hours. Rowland has shown that when an electrified body rotates it behaves as though currents circulated round it, and in this way the earth's magnetism might be explained.

This action of the sun must, however, be slight, owing to its great distance and to the comparatively small size of the earth; hence we may assume with the author that the tension of the earth's charge is very small,—so small as to be unable to pass directly to the air,—but it would be carried off by the evaporated water, and thus the whole air would become charged. An explanation of thunderstorms may be found in this theory, which is supported by the fact that storms are most frequent and most violent in equatorial regions, where also the evaporation is greatest. It is also to be noted that though the charge taken up from the earth by vapour may, nay, must be, of extremely low tension, yet by the condensation of the vapour two effects will follow—the charge will be also condensed, and the conductivity of the vapour greatly increased; so that a very powerful discharge becomes not impossible, but very probable. It may be possible to go even a step further, and to assume with some ground of probability that the moon also acts inductively on the earth.

The author concludes by urging the deep necessity for more extended means of observing atmospheric electricity, by which perhaps it might be determined, amongst other things, whether the sun does really break up the earth's electricity, so that the daylight half is negative and the night half positive.

L. WEBER.—PHOTOMETRY OF DIFFERENTLY COLOURED SOURCES OF LIGHT.

(*Elektrotechnische Zeitschrift, B.* 5, *No.* 4, *April*, 1884, *p.* 166.)

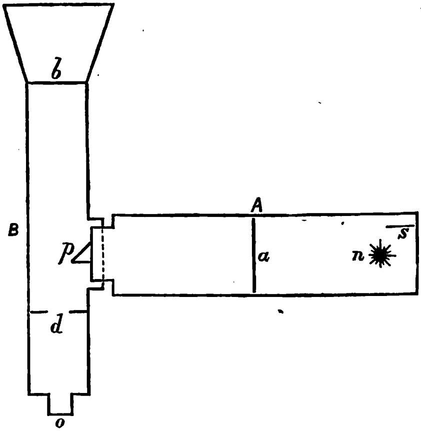
The methods of photometry most usually adopted for the comparison of two lights fail as soon as these are differently coloured. There are, however, two methods not open to the same objections: the one which the author calls the method of equally-illuminated surfaces, and the other the method of lighting power or clearness of definition. By the former it is ascertained how many standard units of light have to be substituted for the experimental light, so that a given surface may be equally illuminated in the two cases. By the latter the number of standard units has to be found which, when they replace the experimental light, will make certain characters or systems of lines equally distinct. The former method has been proposed and carried out by M. Macé de Lépinay, an abstract of whose article in the *Comptes Rendus* appears in the present number of this Journal.

Turning to the second method, which is more especially recommended by Herr L. Weber, we find that it has already been recommended by Dr. W. Siemens and by MM. Crova and Lagarde. Very finely ruled lines, similar to those of a diffraction grating, were placed in the slit of a spectroscope, and only became distinctly visible for a certain fixed amount of illumination. Assuming the intensity of some one colour arbitrarily as unity, it was then possible, as in Lépinay's experiments, to determine the coefficients expressing the quantity by which the impinging light had to be multiplied, in order that for any other colour the grating might appear equally distinct. The author has attempted to turn this method of observation to practical account, by investigating under what conditions the illuminating power of a source of light may be expressed by the formula $B = K I_r$, in which I_r is the intensity of any colour or mixture of colours which to the eye appear monochromatic, and K is a physiological constant depending on the colour corresponding to the determination of I_r, on the general colour of the experimental light, and on the illumination of a given surface on which are extremely fine lines or circles used as test objects.

The apparatus used is shown in the diagram, and is very simple. The tube A, having at one end the standard light *n*, is fixed horizontally, while B can be rotated round the common joint in a vertical plane; *a* is a ground-glass screen, which can be moved along the axis of A, such movement being read off on a scale; *b* is another screen fixed in B, and *d* a diaphragm, while by means of the totally reflecting prism, *p*, both screens can be seen in juxtaposition by an eye placed at *o*. The standard light used was a benzine lamp, the height of the flame being kept constant by observations of a scale, S. In order to obtain monochronism, a glass reddened with copper oxide was placed before the eye-piece at *o*. The value of I_r was then determined for a large number of incandescence lamps.

To determine the value of K, the author used two ground-glass plates on which were photographed identical objects. Each plate was divided into eight

equal compartments, in two rows of four each, and in each compartment was drawn a series of concentric circles alternately black and white, the width of the lines in each compartment gradually diminishing, and hence the number of circles increasing. The width of the lines in cm. was 0·55, 0·50, 0·45, 0·40, 0·35, 0·30, 0·25, 0·20. By the photographic process these dimensions were reduced $\frac{1}{20}$, so that the width of the finest lines became $\frac{1}{10}$ of a mm. Two observations were then made after the two glass screens had been put in position, one at *a*, the other at *b*: first, the plate *a* was placed at 24·5 cm. from *n*, a position which had been previously determined once for all as being the one at which the two images were seen at *o* under the same angle; B was then directed towards the experimental light, and the spectroscope was moved backwards and forwards until the lines on the corresponding squares of the



two plates just became indistinct. The red glass used to determine I_r is then placed before *o*, and the parts of the fields which are not covered by the two plates are brought to an equal degree of illumination by shifting the screen *a*. Suppose it has to be moved *r* cm. from *n*, then

$$K = \frac{r^2}{24{\cdot}5^2}.$$

The author made a second series of observations with a green glass, and observed the new distance, *e*, of *a* from *n*. We have then

$$\frac{G}{R} = \frac{24{\cdot}5^2}{e^2}.$$

The red glass employed allowed rays to pass of wave-lengths between 687 and 680, and the green between 577 and 516.

The results are clearly set out in a series of tables which are too extensive for reproduction.

Dr. A OBERBECK—CHANGES OF MAGNETISM WHEN ACTED UPON BY ALTERNATING CURRENTS.

(*Elektrotechnische Zeitschrift, B. 5, May,* 1884, *p.* 195.)

The theory of Poisson, that the magnetic moment is proportional to the magnetising force, cannot be held as strictly correct, since, on this assumption, when the current ceases to circulate in the coils of an electro-magnet, the magnetism should entirely disappear; the core, however, remains slightly magnetic. This residual magnetism is put down as due to the coercive force of the iron. The question of the molecular retardation is complicated by the fact that induction currents, which are opposed in direction to the magnetising current, are set up in the mass of the iron itself.

The author has formulated three questions: Does the magnetism, irrespective of the above-mentioned induction currents, immediately correspond to the magnetising force, or are there some other causes of retardation? Do different kinds of iron and steel behave themselves differently in this respect? Does the magnetism reach the same intensity with rapidly alternating currents as with continuous ones, or as with slow undulatory currents? The experiments undertaken to answer these queries form the bulk of the paper.

The alternating currents used were produced by a so-called sine-inductor, and the experiments were made both with coils longer than the enclosed cores, and with coils which were much shorter than the cores. As a measure of the magnetism, the currents induced in a second coil were taken; the instruments used, since these currents were also alternating, was a Siemens electro-dynamometer. In a Wheatstone bridge arrangement, the two comparison branches were constituted by a few turns of German silver wire of equal resistance, in the third side was a box of resistances, and in the fourth the experimental coil with its core; the battery diagonal contained the sine-inductor and the fixed coil of the electro-dynamometer, while the usual galvanometer was replaced by the swinging coil of the same instrument. The core of the experimental coil having been removed and the sine-inductor set to work, the resistance in the box was changed until there was no current in the swinging coil of the electro-dynamometer. The iron core was then inserted and a fresh determination made, when from the added resistance necessary to reduce the current to zero, and from the number of alternations of the current, it was possible to obtain a measure of the magnetic moment of the core.

In order to ascertain if there was any retardation in the magnetisation of the core, another arrangement was made. There were two complete closed circuits: the one contained the experimental coil with its core, the sine-inductor, and the fixed coil of the electro-dynamometer; the other, the swinging coil, a resistance, and a coil which surrounded the experimental one, like the secondary of an induction coil. If now there is a difference of phase of $\frac{1}{2}\pi$ between the inducing and the induced currents, the electro-dynamometer will not be deflected; but if on inserting the iron core a deflection is observed, then a retardation of phase must have occurred.

Various cores were experimented upon, and it was found that the retardation was a function of the diameter of the single wires or rods employed, and increased with an increase in this diameter. Iron cores produced more retardation than steel cores. With bundles of wire it appears that the magnetic moment increases slowly with increasing number of alternations. With cores showing greater retardation, the magnetic moment decreases in inverse proportion to the increase of the alternations, from which we may conclude that the alternations follow so quickly that the magnetic moments cannot reach the values corresponding to the constant force.

A further series of experiments was made, in which the two coils were placed some distance apart on the same core, the rest of the arrangement being the same as described above, in order to see how the results would be affected by the magnetic induction acting along the iron core.

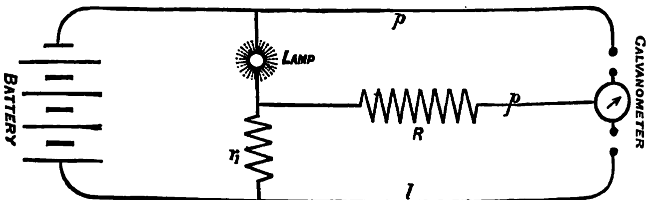
The author concludes that all these phenomena may be explained if we assume that the magnetism follows the same laws with rapidly alternating currents (50 to 180 times a second) as with continuous currents, due heed being taken of the induction currents in the interior of the core itself.

OTTO SCHUMANN—COLOUR AND BRILLIANCY OF GLOW LAMPS.

(*Elektrotechnische Zeitschrift, B.* 5, *No.* 5, *May*, 1884, *p.* 220.)

The determination of the colour was made by means of one of Glan's spectro-photometers, green light of wave-length 557 being taken as unity. The intensity of the light was determined by the method described by Professor Leonhard Weber (see *ante.*) The light used as comparison was the red light emitted by a standard spermaceti candle, all rays above 687 and below 626 being cut off by a red glass. The plane of the carbon filament was placed at an angle of 45 degrees to the axis of the photometer. The height of the flame of the candle was 4·5 cm.

The electrical measurements were made by the plan shown in the figure.



R is a large resistance of from 20,000 to 60,000 S.U., and r_1 one of 3·2 S.U. (In these experiments the Siemens unit was taken = 0·95 ohm.)

To measure the E.M.F., the two branches p were closed and l was opened, the current i_1 was then read on the calibrated galvanometer, whence $p = i_1$ R. Now interrupt the upper circuit p and close l, then the current in the lamp will be $i = i_2 \frac{R + r_1}{r_1}$, and the resistance of the lamp $r = \frac{p}{i}$.

The intensity for red light of any lamp divided by the area of the surface

of the filament must be a constant quantity, if the lamps are at the same point of incandescence. Further, the work done by the current must be equal to the kinetic energy lost by radiation. Assuming an equal emissive power for several lamps, we have in the first instance

$$\frac{L}{A} = \text{constant},$$

and in the second,

$$\frac{W}{A} = K, \text{ or } \frac{L}{W} \times K = \text{constant}.$$

The relation between the light and the work done by the current has been given by Götz by the two equations

$$L = c\,W^3 \text{ and } I = a\,W + b\,W^2,$$

of which the latter is more correct.

The author has determined the constants in the above equations, and gives these values:—

Lamp.	$a.$	$b.$	$c.$
Swan...	— 0·1017	0·005076	0·0000573
Large Edison	— 0·1098	0·003562	0·00006228
Small Edison	— 0·05924	0·00350	0·0000375
Greiner & Friedrichs, No. I.	— 0·03504	0·00577	0·000194
Small Do. No. II.	— 0·1272	0·00622	0·0000907

It is remarkable that the constant b does not change much for the same make of lamp, while a varies considerably according to the size. The resistance of the lamps in all cases varied about the same, and was always, when hot, about half the resistance when cold.

All the results obtained are shown in an exhaustive series of tables.

Dr. IGNAZ KLEMENCIC—DETERMINATION OF v.

(Sitzungbericht der K. Akad. d. Wissensch, Vol. 89, Part 2, Feb., 1884.)

The author commences by recapitulating the values already found, which it may be convenient to reproduce.

In the earlier experiments, Weber and Kohlrausch found $3{\cdot}107 \times 10^{10}$, Thomson found $2{\cdot}82 \times 10^{10}$, Maxwell found $2{\cdot}88 \times 10^{10}$. The values of later experiments all lie between the first and the last of these values.

In 1873, McKichan measured the E.M.F. in both electrostatic and electromagnetic units, and found $v = 2{\cdot}93 \times 10^{10}$.

In 1881, R. Shida, by the same method, found $v = 2{\cdot}995 \times 10^{10}$.

In 1879, Ayrton and Perry, by measuring the capacity of a condenser, found $v = 2{\cdot}98 \times 10^{10}$.

Hockin, by the same method, found $v = 2{\cdot}988 \times 10^{10}$.

In 1881, Stoletow, by a similar method, found $v = 2{\cdot}98 \times 10^{10}$ and $= 3{\cdot}00 \times 10^{10}$.

In 1883, J. J. Thomson determined the quantity of electricity sent by a Daniell cell in the unit of time through a known resistance, and found $v = 2{\cdot}963 \times 10^{10}$

In 1882, F. Exner, by determination of the units of E.M.F., found $v = 3{\cdot}01 \times 10^{10}$. In this value the Siemens unit is taken as equal to $0{\cdot}9717 \times 10^9$ C.G.S. units, and by the newer value $0{\cdot}9433 \times 10^9$; this value of v becomes $2{\cdot}92 \times 10^{10}$.

Rowland, from his experiments, derived the value $v = 3{\cdot}0418 \times 10^{10}$.

The author now gives the results of his own experimental method.

One pole of a battery of E.M.F., E, is joined to a condenser of capacity C, which can be calculated in electrostatic units, the other pole being to earth. If the connection with the condenser is broken several times in a second, and connection with a galvanometer made, we shall obtain a constant deflection, a, and we shall have

$$I = N E C = G a \quad \dots \quad \dots \quad \dots \quad \dots \quad (1)$$

where N is the number of interruptions per second, and G the galvanometer constant.

Now join up the battery to G through a resistance R (known), then

$$I = \frac{E}{R} = G \phi \quad \dots \quad \dots \quad .. \quad \dots \quad (2)$$

or if a shunt, r, is used, and e is the resistance of the galvanometer, B that of battery, we must put

$$R = \frac{B(r + e) + re}{r} \quad \dots \quad \dots \quad \dots \quad \dots \quad (3)$$

Substituting in (2) the value of E in (1), we have

$$R = \frac{a}{\phi N C} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (4)$$

in which R is measured in electrostatic units; and we know

$$R\,(\text{electrostatic}) = \frac{R\,(\text{electro-magnetic})}{v^2}$$

hence finally,

$$v^2 = R_{elm} \times N C \frac{\phi}{a} \quad \dots \quad \dots \quad \dots \quad \dots \quad (6)$$

If we use a differential galvanometer, with two exactly compensated coils, and keep the battery closed through one, while the condenser is discharged through the other, then by properly adjusting B and e we can bring the galvanometer needle to zero; and since the same battery simultaneously gives the constant current and charges the condenser,

$$v = \sqrt{R_{elm} \times N C}.$$

The exact and minute description of the actual apparatus used is too voluminous for reproduction; but it should be added that the interruptions were made by means of a tuning-fork, the resistances used were Siemens units, and for conversion into absolute measure the factor $0{\cdot}941 \times 10^9$ was used, since 1 B.A. unit was found equal to 1·0494 S.U. as used, and the value of the B.A. unit was taken at $0{\cdot}987 \times 10^9$. The galvanometer used was a mirror one of Wiedemann's pattern, with two coils each of 8,000 turns of copper wire. The condenser consisted of two circular plates of steel placed horizontally and supported on a glass pillar, with a glass plate between them. The size of the condenser plates was determined by measuring their circumference with a piece of very thin paper, and the thickness of the glass by a

spherometer. The capacity of the condenser in electrostatic measure was calculated from Kirchoff's formula,

$$C = \frac{R^2}{4\delta} + \frac{R}{4\pi}\left\{\log.\frac{16\pi(\delta+b)R}{e\,\delta^2} + \frac{b}{\delta}\log.\frac{\delta+b}{b} + 2\right\}.$$

As a mean of a large series of observations, the author found

$$v = 3{\cdot}0188 \times 10^{10},$$

the lowest being $3{\cdot}0148 \times 10^{10}$, and the highest $3{\cdot}0240 \times 10^{10}$.

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JOURNAL

OF THE

SOCIETY OF

Telegraph-Engineers and Electricians.

Founded 1871. *Incorporated* 1883.

VOL. XIII. 1885. No. 54.

The One Hundred and Thirty-seventh Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 13th, 1884—Professor W. GRYLLS ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The PRESIDENT: I have to announce to the Society that the Council have seen fit to elect Dr. Edward Davy as an Honorary Member of the Society, in recognition of his early inventions and investigations in connection with the electric telegraph.

The meeting unanimously signified its approval of the action of the Council.

The PRESIDENT: The Society will have heard with great regret of the death, during the recess, of Mr. Robert Sabine, an original member and first Treasurer of the Society; and I am sure the members will wish to join the Council in offering an expression of their sympathy to Mrs. Sabine in the sad loss which has befallen her.

The President's suggestion was unanimously approved.

Donations to the Library of the Society were announced as having been recèived during the recess from the following:—W. Lynd, Associate; H. R. Kempe, Member; Institution of Civil Engineers; G. Gore, Esq.; Brigadier-General S. V. Benét;

C. L. Madsen, Foreign Member; W. D. Gooch, Associate; Hyde Clark, Esq.; United Telephone Company; W. H. Bailey, Esq.; Astronomer-Royal; C. Todd, C.M.G., Member; J. T. Sprague, Member; J. Aylmer, Member; Hon. C. A. Sumner; Mons. G. A. Cassagnes; W. H. Preece, F.R.S., Past-President; Ensign F. J. Sprague; Radcliffe Library; James Sivewright, Member; Major H. F. Turner, R.E., Member; United States Ordnance Department; American Academy of Arts and Sciences; University College; R. Price-Williams, Esq.; Société Météorologique de France; Willoughby Smith, Past-President; Dr. K. E. Zetzsche, Foreign Member; Colonel Sir F. Bolton, Hon. Sec.; Sir C. E. Bright, Vice-President; Institution of Mechanical Engineers; J. A. Fahie, Esq., Member; Professor Fleeming Jenkin, F.R.S., Member; to all of whom a vote of thanks was accorded.

The following paper was then read:—

ON THE THEORY OF ALTERNATING CURRENTS, PARTICULARLY IN REFERENCE TO TWO ALTERNATE-CURRENT MACHINES CONNECTED TO THE SAME CIRCUIT.*

By J. Hopkinson, F.R.S.

In my lecture on Electric Lighting, delivered before the Institution of Civil Engineers last year, I considered the question of two alternate-current dynamo machines connected to the same circuit, but having no rigid mechanical connection between them, and I showed that, if two such machines be coupled in series, they will tend to nullify each other's effect; if parallel, to add their effects. The subject is one which already has practical importance and application, and may have much more in the future; it is also

* *22nd Nov.*, 1884.—My attention has only to-day been called to a paper by Mr. Wilde, published by the Literary and Philosophical Society of Manchester, December 15th, 1868, also *Philosophical Magazine*, January, 1869. Mr. Wilde fully describes observations of the synchronising control between two or more alternate-current machines connected together. I am sorry I did not know of his observations when I lectured before the Institution of Civil Engineers, that I might have given him the honour which was his due. If his paper had been known to those who have lately been working to produce large alternate-current machines, it would have saved them both labour and money.

one suited for discussion, and upon which discussion is desirable. I therefore venture to bring before the Society what I said in my lecture, some other ways of looking at the same subject, and an experimental verification, together with solutions of other problems requiring similar treatment.

The general explanation amounting to proof, so far as machines in series are concerned, is given in the following extract from my lecture:—

"There remains one point of great practical interest in connection with alternate-current machines, How will they behave when two or more are coupled together, to aid each other in doing the same work? With galvanic batteries, we know very well how to couple them, either in parallel circuit or in series, so that they shall aid, and not oppose, the effects of each other; but with alternate-current machines, independently driven, it is not quite obvious what the result will be, for the polarity of each machine is constantly changing. Will two machines coupled together run independently of each other, or will one control the movement of the other in such wise that they settle down to conspire to produce the same effect, or will it be into mutual opposition? It is obvious that a great deal turns upon the answer to this question, for in the general distribution of electric light it will be desirable to be able to supply the system of conductors from which the consumers draw by separate machines, which can be thrown in and out at pleasure. Now I know it is a common impression that alternate-current machines cannot be worked together, and that it is almost a necessity to have one enormous machine to supply all the consumers drawing from one system of conductors. Let us see how the matter stands. Consider two machines independently driven, so as to have approximately the same periodic time and the same electro-motive force. If these two machines are to be worked together, they may be connected in one of two ways: they may be in parallel circuit with regard to the external conductor, as shown by the full line in Fig. 6—that is, their currents may be added algebraically and sent to the external circuit; or they may be coupled in series, as shown by the dotted line—that is, the whole current

may pass successively through the two machines, and the electro-motive force of the two machines may be added, instead of their currents. The latter case is simpler. Let us consider it first. I am going to show that if you couple two such alternate-current machines in series, they will so control each other's phase as to nullify each other, and that you will get no effect from them; and, as a corollary from that, I am going to show that if you couple them in parallel circuit, they will work perfectly well together, and the currents they produce will be added; in fact, that you cannot drive alternate-current machines tandem, but that you may drive them as a pair, or, indeed, any number abreast. In diagram Fig. 7, the horizontal line of abscissæ

FIG. 6.

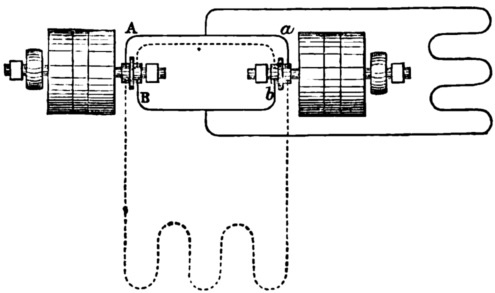
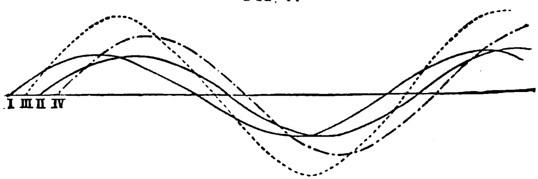


FIG. 7.



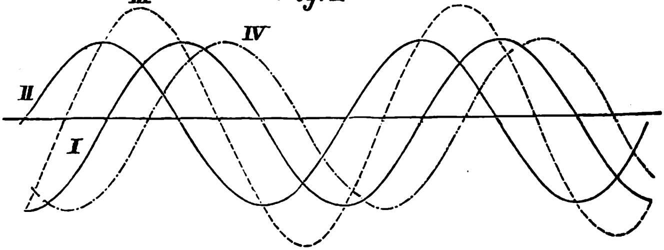
represents the time advancing from left to right; the full curves represent the electro-motive forces of the two machines not supposed to be in the same phase. We want to see whether they will tend to get into the same phase or to get into opposite phases. Now, if the machines are coupled in series, the resultant electro-motive force on the circuit will be the sum of the electro-

motive forces of the two machines. This resultant electro-motive force is represented by the broken curve III; by what we have already seen in Formula IV., the phase of the current must lag behind the phase of the electro-motive force, as is shown in the diagram by curve IV, thus ——.——.——. Now, the work done in any machine is represented by the sum of the products of the currents and of the electro-motive forces, and it is clear that as the phase of the current is more near to the phase of the lagging machine II than to that of the leading machine I, the lagging machine must do more work in producing electricity than the leading machine; consequently its velocity will be retarded, and its retardation will go on until the two machines settle down into exactly opposite phases, when no current will pass. The moral therefore is, Do not attempt to couple two independently driven alternate-current machines in series. Now for the corollary. A B, Fig. 6, represent the two terminals of an alternate-current machine; *a b* the two terminals of another machine independently driven. A and *a* are connected together, and B and *b*. So regarded, the two machines are in series, and we have just proved that they will exactly oppose each other's effects—that is, when A is positive, *a* will be positive also; when A is negative, *a* is also negative. Now, connecting A and *a* through the comparatively high resistance of the external circuit with B and *b*, the current passing through that circuit will not much disturb, if at all, the relations of the two machines. Hence, when A is positive, *a* will be positive, and when A is negative, *a* will be negative also; precisely the condition required that the two machines may work together to send a current into the external circuit. You may therefore, with confidence, attempt to run alternate-current machines in parallel circuit for the purpose of producing any external effect. I might easily show that the same applies to a larger number: hence, there is no more difficulty in feeding a system of conductors from a number of alternate-current machines than there is in feeding it from a number of continuous-current machines. A little care only is required that the machine shall be thrown in when it has attained something like its proper velocity. A further corollary is that alternate currents with

alternate-current machines as motors may theoretically be used for the transmission of power."*

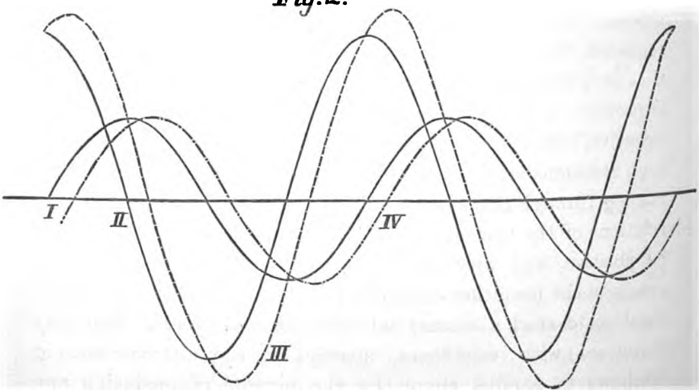
Although the proof of this corollary regarding motors is

Fig. 1



similar to what we have just been going through, it may be instructive to give it. In the accompanying diagrams the full lines I and II represent the electro-motive forces of the two

Fig. 2.



machines (generator and receiver); the dotted line, curve III (- - - - -), the resultant electro-motive force; and the curve IV, the resulting current, each in terms of the time, as abscissæ. The

* "Of course, in applying these conclusions, it is necessary to remember that the machines only *tend* to control each other, and that the control of the motive power may be predominant, and *compel* the two or more machines to run at different speeds."

only difference between the two diagrams is, that in Fig. 1 the two machines have equal electro-motive forces, whilst in Fig. 2 *the receiving machine has double the electro-motive force of the generator.* In both figures the receiving machine lags behind the phase of direct opposition to the generator by one-quarter of a period, or something less. Now observe, the resultant electro-motive force must be in phase behind the receiver, but in advance of the generator. Also observe, the current must be in phase behind the resultant electro-motive force, and may be one-quarter of a period behind, provided only the self-induction be large enough compared with the resistance. The current will then be less than a quarter period behind the generator. This machine will do work upon the current, but the current will be more than a quarter period behind the receiving machine; therefore in the receiver the current does work upon the machine.

The subject is illustrated by the following problems. Of course any of them may be treated more generally by considering the machines as unequal, or by introducing other periodic terms, but I do not see that this would throw more light on the subject:—

I. *Two alternate-current machines, equal in all respects, are connected in series and independently driven at the same speed, to determine the current, etc., in each.*

Let γ be the coefficient of self-induction of each, r the resistance, x the current at time t, and E sin. $\frac{2\pi}{T}(t+\tau)$ and E sin. $\frac{2\mu}{T}(t-\tau)$ the electro-motive forces. Then regarding the coefficient of self-induction as constant, which it is not exactly, and neglecting the effect of currents other than those in the copper wire, the equation of motion is

$$2\,\gamma\,x' + 2\,r\,x = \mathrm{E}\left\{\sin.\frac{2\,\pi}{\mathrm{T}}(t+\tau) + \sin.\frac{2\,\pi}{\mathrm{T}}(t-\tau)\right\}$$

or
$$\gamma\,x' + r\,x = \mathrm{E}\sin.\frac{2\,\pi\,t}{\mathrm{T}}\cos.\frac{2\,\pi\,\tau}{\mathrm{T}}$$

whence
$$x = \frac{\mathrm{E}\cos.\dfrac{2\,\pi\,\tau}{\mathrm{T}}}{r^2 + \left(\dfrac{2\,\pi\,\gamma}{\mathrm{T}}\right)^2}\left\{r\sin.\frac{2\,\pi\,t}{\mathrm{T}} - \frac{2\,\pi\,\gamma}{\mathrm{T}}\cos.\frac{2\,\pi\,t}{\mathrm{T}}\right\}$$

Work done by the leading machine per second

$$= \frac{E^2 \cos. \dfrac{2\pi\tau}{T}}{2\left\{r^2 + \left(\dfrac{2\pi.\gamma}{T}\right)^2\right\}} \cdot \left\{r \cos. \frac{2\pi\tau}{T} - \frac{2\pi\gamma}{T} \sin. \frac{2\pi\tau}{T}\right\}$$

$$= \frac{E^2}{4\left\{r^2 + \left(\dfrac{2\pi\gamma}{T}\right)^2\right\}} \cdot \left\{r\left(1 + \cos. \frac{4\pi\tau}{T}\right) - \frac{2\pi\gamma}{T} \sin. \frac{4\pi\tau}{T}\right\}$$

From this at once follows that the leading machine does least work, and will tend to increase its lead until $\tau = \frac{T}{4}$, when the two machines will neutralise each other, as already proved geometrically. *The leading machine may actually become a motor and do mechanical work, although its electro-motive force is precisely equal to that of the following machine.*

Considering the important case when r is neglible, we have

$$x = -\frac{E \cos. \dfrac{2\pi\tau}{T} \cdot \cos. \dfrac{2\pi t}{T}}{\dfrac{2\pi\gamma}{T}},$$

$$\text{rate of working} \qquad = \frac{E^2 \sin. \dfrac{4\pi\tau}{T}}{4 \cdot \dfrac{2\pi\gamma}{T}}.$$

This is a maximum when $\tau = \frac{T}{8}$, and then it is equal to one-half of the maximum work which can be obtained from either machine when connected to a resistance only, which occurs when that resistance is $\frac{2\pi\gamma}{T}$; the current, however, is the same as when the maximum work is being done on resistance, and is $\frac{1}{\sqrt{2}}$ of the current the machine will give if short-circuited. The difference of potential between the two leads connecting the machines, whether $r = 0$ or not, is $E \cos. \frac{2\pi t}{T} \sin. \frac{2\pi\tau}{T}$. If there be no work done on the receiving machine and $r = 0$, $\tau = \frac{T}{4}$, and the amplitude of the difference of potential between the leads is E;

if, on the other hand, the maximum work is being transmitted, the potential measured will be $\frac{1}{\sqrt{2}}$ of that observed when either machine is run on open circuit.

II. *Two machines are coupled parallel and connected to an external circuit resistance* R.

Let x_1 x_2 be currents in the two machines. The external current will be x_1+x_2, and consequently the difference of potential at the junction, $R\,(x_1+x_2)$.

Let the electro-motive forces of the two machines regarded in this case as connected parallel be $E \sin. \frac{2\pi\,(t\pm\tau)}{T}$, and let the self-induction and resistance of each be $2\,\gamma$ and $2\,r$.

The equations of motion then are

$$2\,\gamma\,x'_1 + 2\,r\,x_1 = E \sin. \frac{2\,\pi\,(t+\tau)}{T} - R\,(x_1+x_2)$$

$$2\,\gamma\,x'_2 + 2\,r\,x_2 = E \sin. \frac{2\,\pi\,(t-\tau)}{T} - R\,(x_1+x_2)$$

whence

$$\gamma\,(x'_1+x'_2) + (R+r)\,(x_1+x_2)$$
$$= E \sin. \frac{2\,\pi\,t}{T} \cdot \cos. \frac{2\,\pi\,\tau}{T}$$

and

$$\gamma\,(x'_1-x'_2) + r\,(x_1-x_2) = E \cos. \frac{2\,\pi\,t}{T} \sin. \frac{2\,\pi\,\tau}{T}$$

Solving these

$$x_1+x_2 = \frac{E \cos. \frac{2\,\pi\,\tau}{T}}{(r+R)^2+\left(\frac{2\,\pi\,\gamma}{T}\right)^2}\left\{(r+R) \sin. \frac{2\,\pi\,t}{T} - \frac{2\,\pi\,\gamma}{T} \cos. \frac{2\,\pi\,t}{T}\right\}$$

$$x_1-x_2 = \frac{E \sin. \frac{2\,\pi\,\tau}{T}}{r^2+\left(\frac{2\,\pi\,\gamma}{T}\right)^2}\left\{r \cos. \frac{2\,\pi\,t}{T} + \frac{2\,\pi\,\gamma}{T} \sin. \frac{2\,\pi\,t}{T}\right\}$$

electrical work done by the leading machine

$$= \tfrac{1}{2}\,E \sin. \frac{2\,\pi\,(t+\tau)}{T}\left\{x_1 + x_2 + (x_1 - x_2)\right\}$$

$$= \tfrac{1}{4}\,\frac{E^2}{(r+R)^2+\left(\frac{2\,\pi\,\gamma}{T}\right)^2}\left\{(r+R) \cos.^2 \frac{2\,\pi\,\tau}{T}\right\}$$

$$-\frac{2\pi\gamma}{T}\sin.\frac{2\pi\tau}{T}\cos.\frac{2\pi\tau}{T}\Big\}$$

$$+\tfrac{1}{4}\frac{E^2}{r^2+\left(\frac{2\pi\gamma}{T}\right)^2}\Big\{r\sin.^2\frac{2\pi\tau}{T}$$

$$+\frac{2\pi\gamma}{T}\sin.\frac{2\pi\tau}{T}\cos.\frac{2\pi\tau}{T}\Big\}$$

This expression shows that *the leading machine does most work in all cases.* Suppose r is small compared with R and $\frac{2\pi\gamma}{T}$, also that $R = \frac{2\pi\gamma}{T}$, we have the work done per second

$$=\frac{E^2}{8R}\left\{\cos.^2\frac{2\pi\tau}{T}+\sin.\frac{2\pi\tau}{T}\cos.\frac{2\pi\tau}{T}\right\};$$

make $\tau = -\frac{T}{8}$, and we see that the following machine will then do no work; when τ exceed this, the following machine becomes a motor and absorbs electrical work.

III. *Suppose the terminals of an alternate-current machine are connected to a pair of conductors, the difference of potential between which is completely controlled by connection with other alternate-current machines.*

Let γ and R be the coefficient of self-induction and the resistance of the machine and its own conductors up to the point at which the potential is completely controlled. Let the difference of potential of the main conductors be A $\sin.\frac{2\pi t}{T}$, and let the electro-motive force of the machine be B $\sin.\frac{2\pi(t-\tau)}{T}$.

Equation of motion is

$$\gamma x' + Rx = B\sin.\frac{2\pi(t-\tau)}{T} - A\sin.\frac{2\pi t}{T},$$

whence

$$x = \frac{1}{R^2+\left(\frac{2\pi\gamma}{T}\right)^2}\left[B\left\{R\sin.\frac{2\pi(t-\tau)}{T}-\frac{2\pi\gamma}{T}\cos.\frac{2\pi(t-\tau)}{T}\right\}\right.$$

$$\left. A\left\{R\sin.\frac{2\pi t}{T}\quad\frac{2\pi\gamma}{T}\cos.\frac{2\pi t}{T}\right\}\right]$$

electrical work done by the machine in unit of time

$$= x\,\mathrm{B}\,\sin.\frac{2\pi(t-\tau)}{\mathrm{T}}$$

$$=\frac{1}{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2}\left[\frac{\mathrm{B}^2\,\mathrm{R}}{2}-\frac{\mathrm{A\,B}}{2}\left\{\mathrm{R}\cos.\frac{2\pi\tau}{\mathrm{T}}+\frac{2\pi\gamma}{\mathrm{T}}\sin.\frac{2\pi\tau}{\mathrm{T}}\right\}\right]$$

if τ be positive, that is, if machine be lagging in its phase, work done is less than if it be negative; hence τ will tend to zero, or the machine will tend to adjust itself to add its currents to that of the system of conductors. The machine may act as a motor *even though its electro-motive force be greater than that of the system*, for let

$$\frac{\mathrm{R}}{\frac{2\pi\gamma}{\mathrm{T}}}=\tan.\frac{2\pi\phi}{\mathrm{T}},$$

work (electric) done by machine

$$=\frac{\mathrm{B}^2\mathrm{R}}{2\left\{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}}-\frac{\mathrm{A\;B}}{2\left\{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}^{\frac{1}{2}}}\sin.\frac{2\pi(\phi+\tau)}{\mathrm{T}}$$

this has a minimum value when $\phi+\tau=\frac{\mathrm{T}}{4}$, and then the mechanical work done by machine or electrical work received by the machine

$$=\frac{\mathrm{B}}{2\left\{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}^{\frac{1}{2}}}\left\{\mathrm{A}-\frac{\mathrm{R\,B}}{\left\{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}^{\frac{1}{2}}}\right\}$$

and this is positive provided

$$\frac{\mathrm{A}}{\mathrm{B}}>\frac{\mathrm{R}}{\left\{\mathrm{R}^2+\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}^{\frac{1}{2}}}$$

There are two or three other problems of sufficient interest to make it worth while giving them here, although not directly relating to alternate-current machines coupled together.

IV. *To determine the law of an alternate current through an electric arc.*

It has been shown by Joubert that in an arc the difference of potential is of approximately constant numerical value, reversing its value discontinuously with the reversal of the current, probably

at the instant of reversal of current. We shall assume, then, that there is in the arc a constant electro-motive force, A, always opposed to the current, except when the current ceases, and that then its value is zero.

The equation of motion is

$$\gamma x' + \mathrm{R}\, x = \mathrm{E} \sin. \frac{2\pi t}{\mathrm{T}} \mp \mathrm{A}$$

the negative sign being taken when x is $+^{ve}$, the positive when x is negative. Solving generally

$$x = \mp \frac{\mathrm{A}}{\mathrm{R}} + \frac{\mathrm{E}}{\left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2 + \mathrm{R}^2}\left(-\frac{2\pi\gamma}{\mathrm{T}} \cos. \frac{2\pi t}{\mathrm{T}} + \mathrm{R} \sin. \frac{2\pi t}{\mathrm{T}}\right) + \mathrm{C}\, e^{-\frac{\mathrm{R}}{\gamma} t}$$

this equation will continuously hold good for a half period from $x = 0$ to $x = 0$ again, but at each half period the arbitrary constant C is changed with the sudden change of sign of A. It is determined by the consideration that, if for a certain value t_0 of t, x should vanish, it shall vanish again when $t = t_0 + \frac{\mathrm{T}}{2}$. This applies to the case when E is sufficiently large, as is practically the case, but if the current should cease for a finite time this condition will be varied, and instead of it we have the condition $x = 0$ when $\mathrm{E} \sin. \frac{2\pi t}{\mathrm{T}} = \mathrm{A}$. This latter case I do not propose to consider further.

Let $$\frac{2\pi\gamma}{\mathrm{R\,T}} = \tan. \frac{2\pi t_1}{\mathrm{T}}$$

$$x = \mp \frac{\mathrm{A}}{\mathrm{R}} + \frac{\mathrm{E}}{\sqrt{\left\{\mathrm{R}^2 + \left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}}} \sin. \frac{2\pi(t-t_1)}{\mathrm{T}} + \mathrm{C}\, e^{-\frac{\mathrm{R}}{\gamma} t}$$

putting $t = t_0$ and $t = t_0 + \frac{\mathrm{T}}{2}$ we have

$$0 = -\frac{\mathrm{A}}{\mathrm{R}} + \frac{\mathrm{E}}{\sqrt{\left\{\mathrm{R}^2 + \left(\frac{2\pi\gamma}{\mathrm{T}}\right)^2\right\}}} \sin. \frac{2\pi(t_0-t_1)}{\mathrm{T}} + \mathrm{C}\, e^{-\frac{\mathrm{R}}{\gamma} t_0}$$

$$0 = -\frac{A}{R} - \frac{E}{\sqrt{\left\{R^2 + \left(\frac{2\pi\gamma}{T}\right)^2\right\}}} \sin. \frac{2\pi(t_0 - t_1)}{T}$$
$$+ C e^{-\frac{R}{\gamma}t_0} \cdot e^{-\frac{RT}{2\gamma}}$$

equations to determine t_0 and C.

Eliminating C,

$$\frac{RE}{A\sqrt{\left\{R^2 + \left(\frac{2\pi\gamma}{T}\right)^2\right\}}} \cdot \sin. \frac{2\pi(t_0 - t_1)}{T} = -\tanh. \frac{RT}{4\gamma}$$

Having obtained t_0, C is given by equation

$$\frac{2A}{R} = C e^{-\frac{R}{\gamma}t_0}\left(1 + e^{-\frac{R}{2\gamma}T}\right)$$

This gives the complete solution of the problem.

A case of special importance is that in which R is small; let us therefore consider the case $R = 0$, the solution then is

$$\gamma x = -\frac{T}{2\pi} E \cos. \frac{2\pi t}{T} - At + C.$$

In the same way as before

$$E \cos. \frac{2\pi t_0}{T} = \frac{A\pi}{2},$$

$$C = A\left(t_0 + \frac{T}{4}\right).$$

The limiting case to which the solution applies is given by $x' = 0$ when $t = t_0 + \frac{T}{2}$.

$$E \sin. \frac{2\pi t_0}{T} = A,$$

whence
$$E^2 = A^2\left(1 + \frac{\pi^2}{4}\right),$$

or
$$A = E \times 0{\cdot}538.$$

Roughly, we may say that, in order that the current may not cease for a finite time, E *must be at least double of* A; A will of course depend upon the length of the arc. The work done in the arc will be proportional to the arithmetical mean value of the current taken without regard to sign. This is of course quite a

different thing from the mean current as measured by an electro-dynamometer. Let us examine what error is caused by estimating the work done in the arc as equal to the current measured by the dynamometer multiplied by the mean difference of potential

The actual work done per second

$$= \frac{2\,A}{T}\int_{t_0}^{t_0+\frac{T}{2}} x\,d\,t$$

$$= \frac{2\,A}{\pi\,\gamma} \cdot \frac{T}{2\,\pi} \cdot \sqrt{E^2 - \frac{\pi^2\,A^2}{4}}.$$

The mean square of the current, as measured by the electro-dynamometer, is

$$\frac{2}{T}\int_{t_0}^{t_0+\frac{T}{2}} x^2\,d\,t = \overline{\frac{T}{2\,\pi\,\gamma}}\Big|^2 \left\{\frac{E^2}{2} - 2\,A^2\right\} + \frac{A^2\,T^2}{48\,\gamma^2},$$

and the work done by this current is apparently the square root of the above expression multiplied by A. It is easy to see that this is greater in all cases than the work done, but it is worth while to examine the extent of the error. If we treated the arc as an ordinary resistance, we should assume work per second

$$= \frac{A}{\gamma}\sqrt{\left(\frac{T}{2\,\pi}\right)^2\left(\frac{E^2}{2} - 2\,A^2\right) + \frac{A^2\,T^2}{48}}.$$

Taking a fairly practical case, assume $A = \frac{2}{5}$ E, we have actual work per second

$$= \frac{A^2\,T}{\gamma} \cdot \frac{1}{\pi^2}\sqrt{\frac{25}{4} - \frac{\pi^2}{4}}$$

$$= \frac{A^2}{\gamma}.\,T\,\frac{\sqrt{15}}{20} \text{ nearly,}$$

work done estimated by electro-dynamometer

$$= \frac{A^2\,T}{\gamma}\sqrt{\frac{1}{40}\left(\tfrac{1}{2}\,\frac{25}{4} - 2\right) + \frac{1}{48}} \text{ nearly}$$

$$= \frac{A^2\,T}{\gamma}\,\frac{1}{20}\sqrt{\frac{235}{12}}$$

or nearly $\frac{1}{8}$ part too much. This will suffice to show that the matter is not a mere theoretical refinement. Another erroneous method of estimating the power developed in an arc is, to replace

by a resistance and adjust this resistance till the current, as measured by an electro-dynamometer, is the same as with the arc, and assume that the work done in the resistance is the same as the work done in the arc.

Returning to the expression

$$\frac{2\,A}{\pi.\gamma} \cdot \frac{T}{2\,\pi} \sqrt{E^2 - \frac{\pi^2\,A^2}{4}}$$

we may enquire, given T A and the dimensions of the machine, how ought it to be wound or its coils connected, that most work may be done in the arc? If the number of convolutions be varied, E will vary as the convolutions, γ as their square, therefore $\gamma \propto E^2$; we are therefore to determine E so that $\frac{1}{E^2} \sqrt{E^2 - \frac{\pi^2\,A^2}{4}}$ is a maximum which occurs when $E = \pi\,A$. When the resistance of the circuit is taken into account, this result will be modified. It suffices to prove that it is desirable that the potential of the machine should be materially in excess of that required to maintain the arc.

V.* In all that precedes it is assumed, not only that γ is constant, but that the copper conductor of the armature is the only conductor moving in the field. If there be iron cores in the armature, we shall approximate to the effect by regarding such cores as a second conducting circuit. Slightly changing the notation, let L be coefficient of self-induction of the copper circuit, N coefficient of self-induction of the iron circuit and R^1 its resistance, I^1 the magnetic induction of the field magnets upon the iron circuit and M the coefficient of mutual induction of the two circuits, y the current in the iron. The equations of motion are obtained from the expression for the energy, viz.,

$$\tfrac{1}{2}\{L x^2 + 2\,M x y + N y^2 - 2\,I x - 2\,I' y\}$$

and are

$$L x' + M y' + R x = \frac{d\,I}{d\,t} = \frac{2\,\pi\,A}{T} \cos. \frac{2\,\pi\,t}{T}$$

$$M x' + N y' + R' y = \frac{d\,I'}{d\,t} = \frac{2\,\pi\,B}{T} \cos. \frac{2\,\pi\,t}{T}$$

* *Vide* also *Encyclopædia Britannica*, article "Lighting."

for in general the iron cores and the copper conductor are symmetrically arranged. Assume

$$x = a \sin. \frac{2\pi t}{T} = b \cos. \frac{2\pi t}{T}$$

$$y = a' \sin. \frac{2\pi t}{T} + b' \cos. \frac{2\pi t}{T}$$

and substitute in the equations of motion, we have the following four equations to determine the constants, $a\ b\ a'\ b'$

$$a \frac{2\pi L}{T} + a' \frac{2\pi M}{T} + R b = \frac{2\pi A}{T},$$

or

$$\left.\begin{aligned} a L + a' M + b \frac{T R}{2\pi} &= A \\ b L + b' M - a \frac{T R}{2\pi} &= 0 \\ a M + a' N + b' \frac{T R'}{2\pi} &= B \\ b M + b' N - a' \frac{T R'}{2\pi} &= 0 \end{aligned}\right\}$$

and

These equations contain the solution of the problem, but are too cumbersome to be worth while solving generally; we will however prove the statements made in the lecture before the Civil Engineers.

1st. Compare short circuit and open circuit, that is, $R = 0$ very nearly, and $R = \infty$. In the former case we find that work done in the iron is diminished, and if $B = \frac{A M}{L}$ we have the paradoxical result that there are no currents induced in the iron of the cores and no work is required to drive the machine. This of course can never actually occur, because R can never absolutely vanish. It suffices to show, however, that the current in the copper circuit may diminish the whole power required to drive the machine, to an amount less than the power required to drive the machine on open circuit.

2nd. The other statement related to the effect of the currents in the iron upon the currents produced in the copper circuit. Assume that the effect is a small one, for a first approximation. Neglect it, that is, treat the currents in the iron and the current

in the copper as independent of each other, and then see how each would disturb the other.

The first approximation then is

$$\left\{\begin{aligned} a &= \frac{\mathrm{A\,L}}{\mathrm{L}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}^2}{4\,\pi^2}}\,; \quad a' = \frac{\mathrm{B\,N}}{\mathrm{N}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}'^2}{4\,\pi^2}} \\ b &= \frac{\mathrm{A}\,\dfrac{\mathrm{T\,R}}{2\,\pi}}{\mathrm{L}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}^2}{4\,\pi^2}}\,; \quad b' = \frac{\mathrm{B}\,\dfrac{\mathrm{T\,R}'}{2\,\pi}}{\mathrm{N}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}'^2}{4\,\pi^2}} \end{aligned}\right.$$

If we substitute these in the general equations as corrections, we have

$$\left\{\begin{aligned} a\,\mathrm{L} + b\,\frac{\mathrm{T\,R}}{2\,\pi} &= \mathrm{A} - \frac{\mathrm{B\,N\,M}}{\mathrm{N}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}'^2}{4\,\pi^2}} \\ -\,a\,\frac{\mathrm{T\,R}}{2\,\pi} + b\,\mathrm{L} &= -\,\frac{\mathrm{B}\,\dfrac{\mathrm{T\,R}'}{2\,\pi}\,\mathrm{M}}{\mathrm{N}^2 + \dfrac{\mathrm{T}^2\,\mathrm{R}'^2}{4\,\pi^2}} \end{aligned}\right.$$

which shows that the disturbing effect of each circuit upon the other is to diminish the apparent electro-motive force, but to accelerate its phase.

VI. A very similar problem is that of secondary generators or induction coils, whether used for the conversion of high potentials to low, or the reverse. To treat it generally, taking the magnetisation of the iron cores, which are always used, as a non-linear function of the currents in the coils, would be a matter of much difficulty; we therefore assume, as is usual, that the co-efficients of induction are constants, noting in passing that this is not strictly the fact, though it is very nearly the fact, when the cores are not saturated and when the lines of magnetic induction pass through non-magnetic space.

Let, then, R, r be the resistances of the primary and secondary circuits,

L coefficient of self-induction of the primary,

N coefficient of self-induction of the secondary,

M coefficient of mutual induction of the two circuits,

x and y the currents in the two circuits at time t,

X the electro-motive force applied in the primary circuit by an alternate-current dynamo machine or otherwise,

the equations of motion will be

$$\left.\begin{array}{l} L\,x' + M\,y' + R\,x = X \\ M\,x' + N\,y' + r\,y = 0 \end{array}\right\}$$

Various assumptions may be made as to X, but that most likely to be adopted in the practical work of secondary generators is that X is kept so adjusted that

$$x = A \cos.\, n\,t \text{ where } n = \frac{2\,\pi}{T}$$

and to enquire how X will depend on the resistances

$$N\,y' + r\,y = n\,A\,M \sin.\, n\,t$$

$$y = \frac{n\,A\,M}{n^2\,N^2 + r^2}\,(-n\,N \cos.\, n\,t + r \sin.\, n\,t)$$

$$X = A\left[\left\{-n\,L + \frac{n^3\,N\,M^2}{n^2\,N^2 + r^2}\right\}\sin.\, n\,t + \left\{\frac{M^2\,n^2\,r}{n^2\,N^2 + r^2} + R\right\}\cos.\, n\,t\right]$$

As in the case of the dynamo machine, the work done in the secondary circuit is greatest when $r = n\,N$. The expression for X serves to show that when the secondary is short-circuited a *lower* electro-motive force of the generating circuit is required than when it is on open circuit. In induction coils the electrostatic capacity of the coils themselves has important effects. An illustration of the effect of electrostatic induction is found in the old-fashioned Ruhmkorff coils. These were not wound symmetrically, but in such wise that one end of the secondary coil was on the whole towards the inside, the other towards the outside of the bobbin. In such coils a spark to earth may be obtained from the outside end, but not from the inside. The reason is that the outer convolutions have smaller electrostatic capacity than the inner ones. The terminals may be made to give equal sparks by the simple expedient of laying a piece of tinfoil around the whole coil and connecting it to earth.

VII. Some time ago Dr. Muirhead told me that he had observed that the effect of an alternate-current machine could be increased by connecting it to a condenser. This is not difficult

to explain: it is a case of resonance analogous to those which are so familiar in the theory of sound and in many other branches of physics.

Take the simplest case, though some others are almost as easy to treat. Imagine an alternate-current machine with its terminals connected to a condenser; it is required to find the amplitude of oscillation of potential between the two sides of the condenser. Let R, γ be the resistance and self-induction of the machine, $E \sin. \frac{2\pi t}{T}$ its electro-motive force, C the capacity of the condenser, V the difference of potential sought, and x the current in the machine, then

$$C V' = x$$

and

$$\gamma x' + R x = E \sin. \frac{2\pi t}{T} - V$$

whence

$$\gamma x'' + R x' = \frac{2\pi}{T} E \cos. \frac{2\pi t}{T} - \frac{x}{C}$$

$$x = \frac{\left\{1 - C\gamma \left(\frac{2\pi}{T}\right)^2\right\} \cos. \frac{2\pi t}{T} + R C \frac{2\pi}{T} \sin. \frac{2\pi t}{T}}{\left\{1 - C\gamma\left(\frac{2\pi}{T}\right)^2\right\}^2 + R^2 C^2 \left(\frac{2\pi}{T}\right)^2} \cdot \frac{2\pi E C}{T}$$

$$V = \frac{\left\{1 - C\gamma \left(\frac{2\pi}{T}\right)^2\right\} \sin. \frac{2\pi t}{T} - R C \frac{2\pi}{T} \cos. \frac{2\pi t}{T}}{\left\{1 - C\gamma\left(\frac{2\pi}{T}\right)^2\right\}^2 + R^2 C^2 \left(\frac{2\pi}{T}\right)^2} \cdot E$$

amplitude of V is therefore

$$= \frac{1}{\sqrt{\left\{1 - C\gamma\left(\frac{2\pi}{T}\right)^2\right\}^2 + R^2 C^2 \left(\frac{2\pi}{T}\right)^2}} \cdot E$$

Now suppose E = 100 volts, the machine would light up an incandescent lamp of about 69 volts; let $T = \frac{1}{200}$ second, C = 100 microfarads, and $\frac{2\pi\gamma}{T}$ = eight ohms, and $R = \frac{1}{10}$ ohm, all figures which could be practically realised, we have amplitude of V = 80 E roughly, or the apparent electro-motive force would be increased eighty-fold.

We now return to the principal subject of the present communication. Some attempts have been made to verify the proposition

that two alternate-current machines can be advantageously connected parallel, but I believe, till recently, without success. I had no convenient opportunity for testing the point myself till last summer, when I had two machines of De Meritens, intended for the lighthouse of Tino,* in my hands. I have made no determinations of the constants of these machines, but between three and four years ago I thoroughly tested a pair of similar machines now in use at a lighthouse in New South Wales.* Each machine has five rings of sixteen sections, and 40 permanent magnets. The resistance of the whole machine as connected for lighthouse work (a single arc) was 0·0313, its electro-motive force (E) when running 830 revolutions per minute, 95 volts and $\frac{2\pi\gamma}{T} = 0{\cdot}443$ ohms. It was further remarked that the loss of power was least with a maximum load, as is shown in the following table :—

Power applied as measured in belt ...	3·1,	4·8,	5·6,	6·5,	5·4
Electric power developed	0·7,	3·4,	4·3,	5·7,	3·4
Mean current in ampères	7·7,	38·6,	51·7,	73·6,	151

This result illustrates well the conclusion arrived at in Problem V. above.

Last summer the two machines for Tino were driven from the same countershaft by link bands, at a speed of 850 to 900 revolutions per minute; the pulleys on the countershaft were sensibly equal in diameter, but those on the machines differed by rather more than a millimètre, one being 300, the other 299 mm. in diameter (about); thus the two machines had not when unconnected exactly the same speed. The pulleys have since been equalised. The bands were of course put on as slack as practicable, but no special appliance for adjusting the tightness of the bands was used. The experiment succeeded perfectly at the very first attempt. The two machines, being at rest, were coupled in series with a pilot incandescent lamp across the terminals, the two bands were then simultaneously thrown on : for some seconds

* The engines, dynamos, lamps, optical apparatus, and lanterns of both these lighthouses have been supplied by Messrs. Chance Bros. & Co.

the machines almost pulled up the engine. As the speed began to increase, the lamp lit up intermittently, but in a few seconds more the machines dropped into step together, and the pilot lamp lit up to full brightness and became perfectly steady and remained so. An arc lamp was then introduced, and a perfectly steady current of over 200 ampères drawn off without disturbing the harmony. The arc lamp being removed, a Siemens electro-dynamometer was introduced between the machines, and it was found that the current passing was only 18 ampères, whereas, if the machines had been in phase to send the current in the same direction, it would have been more than ten times as great. On throwing off the two bands simultaneously, the machines continued to run by their own momentum, with retarded velocity. It was observed that the current, instead of diminishing from diminished electro-motive force, steadily increased to about 50 ampères, owing to the diminished electrical control between the machines, and then dropped off to zero as the machines stopped. Professor Adams will, I hope, give an account of experiments he has tried with me, and on other occasions, at the South Foreland. With De Meritens' machines, I regard coupling two or more machines parallel as practically the best way of obtaining exceptionally great currents when required in a lighthouse for penetrating a thick atmosphere.

The President: Perhaps it will be convenient to members that I should give the communication set down in my name before the discussion on Dr. Hopkinson's paper takes place, so that the discussion may be taken on the two papers at the same time. I will therefore ask Professor Foster to take the chair during the reading of my paper.

Professor G. C. Foster having taken the chair, the following paper was then read:—

THE ALTERNATE-CURRENT MACHINE AS A MOTOR.

By Professor W. Grylls Adams, F.R.S., President.

In July last, whilst engaged in testing the efficiency of alternate-current magneto machines at the South Foreland, for the Elder Brethren of the Trinity House, it was suggested to me

by Dr. Hopkinson to try the experiment, which was shown by him to be possible in his lecture before the Institution of Civil Engineers last year, viz., the running of two alternate-current magneto machines together, without clutching them on the same axis, in order to supply current to an external circuit.

The machines now being tested at the South Foreland are three De Meritens alternate-current magneto machines of the largest size made by him, having 60 permanent magnets, arranged in 5 rings of 12 magnets and 24 coils on each ring of the revolving armature. Each coil consists of four layers of wire, and is 27 millimètres deep and about 100 millimètres wide. The 120 coils when arranged in parallel circuit for lighthouse work have a resistance of about one-twentieth of an ohm. The internal resistances of the three machines are ·044 ohms, ·049 ohms, and ·056 ohms.

The five rings of coils on each of the machines are connected in parallel circuit when all the brushes are down, but the collectors and brushes on each machine are made in three sections, so that it is possible to employ one ring only of magnets and coils, or two rings only, or the remaining three rings, or the whole five rings together, by bringing the respective brushes in contact with their own collectors, which are insulated from one another by ebonite rings or flanges on the ebonite axis which insulates the collectors from the axis of the machine. The diameter of the armature carrying the revolving coils is 2 ft. 6 in., or 760 millimètres, and the driving pulley is about 16 inches, or 400 millimètres, in diameter.

When revolving at the normal speed, 600 revolutions per minute, the magneto machines have been found to work with very great steadiness and with great efficiency. The electromotive force is about 75 volts and $\frac{2\pi\gamma}{T} = \cdot 3$ ohms nearly. When running with the circuit open, a very considerable amount of work is spent on the friction and internal work within the machine, but on closing the circuit this is greatly diminished, and the output of the machines, or the electrical energy in the external circuit, exceeds the *additional* work done to produce it. Thus a single

machine absorbs 4·2 horse-power when running at 600 revolutions with the circuit open, yet only absorbs 10·4 horse-power with the circuit closed with one arc lamp in circuit, when it yields in electrical energy 6,000 watts, or 8 horse-power, in the external circuit. These watts are obtained by multiplying the average electro-motive force, as given by Cardew's voltmeter, by the average current as given by Siemens' electro-dynamometer. This product will give a result which, according to Dr. J. Hopkinson's calculations, is above the true value.

The electro-motive force was also determined by a quadrant electrometer, by joining the needle to one pair of quadrants, when the expression for the electro-motive force becomes $(V - V_1)^2 = E^2 = \frac{2\,d}{k}$, where d is the deflection, and k the constant of the electrometer.

The method of determining the work done in the arc by joining the pairs of quadrants of the electrometer to the two ends of a known resistance placed in the circuit, according to the formula

$$d_1 = k\,(V_1 - V_2)\left(V - \frac{V_1 + V_2}{2}\right) = k\,C\,r\left(V - \frac{V_1 + V_2}{2}\right),$$

was also tried, but the introduction of even a small resistance, r, materially altered the electro-motive force and also the current in the circuit, and the method was not sufficiently sensitive. Thus, when the deflection by the first method, or $\frac{k}{2}(V - V_1)^2$, was 120 divisions of the scale for an electro-motive force of 57 volts, the deflection by the second method was only about 30 divisions when r was about one-fifteenth of an ohm. The addition of this resistance considerably increased the electro-motive force, and diminished the current in the circuit.

At the South Foreland the leads from the machines to the arc lamps in the experimental tower have a resistance of ·066 of an ohm, and when two machines were clutched together and working in series, so as to supply two arcs in series, the yield of electrical energy per second was about 10,200 watts in the external circuit; whereas, when working in parallel circuits through the

same leads to one arc lamp, the yield was only about 8,600 watts in the external circuit.

To test whether alternate-current machines would work in harmony without being rigidly made fast on the same axis, two machines which are usually clutched together were set running, being joined in parallel circuit, and when they had attained their usual speed of about 600 revolutions a minute they were unclutched, and each was driven only by its own belt. The two machines continued to run together very steadily, mutually governing one another; the electro-motive force remained steady at 75 volts on open circuit, the same as the electro-motive force of one of the machines which had been previously tested.

The machines were not quite equal in efficiency, for before this experiment one of the single machines had given a current of 135 ampères, and the other 165 ampères, with electro-motive force of 37 volts, at the normal speed of 600 revolutions; now on closing the circuit from the two machines through the arc, the electro-motive force became steady at 40 volts, and there was a current of 221 ampères, giving 8,800 watts in the external circuit, and the machines continued to run as steadily as if they had been clutched.

The lamp was now put out of the circuit and the machines short-circuited upon one another, without the slightest danger to the machines, owing to their high coefficient of self-induction. The belt was then thrown off one machine, when it continued to run at the same uniform speed, being driven as a motor by the electric current from the other single machine, and the electro-motive force was equal to the electro-motive force of the machine on open circuit.

An attempt was now made to introduce an arc into the circuit, but the motor immediately began to lose its speed and the arc became fitful with long beats, as in two musical instruments getting out of harmony, the beats increasing as the speed fell and the machine stopped.

A third machine similar to the others, but not placed on the same axis and having no mechanical connection with them, was also driven as a motor by one of the other two machines.

These experiments were repeated, and some other interesting experiments made, on October 18th, when Dr. J. Hopkinson went with me to the South Foreland.

Two machines were started together, but independently, each being driven by its own belt, and they increased their speed in harmony, always going together until they reached their normal speed, when the electro-motive force rose to 80 volts. The belt was now thrown off one of the machines, when the electro-motive force at the terminals of the driving machine remained at 80 volts. A strap was placed round the pulley of the motor machine, and a weight of 14 lbs. was hung on the strap; the machines continued to run together, but the electro-motive force oscillated from 80 to 78 volts as the weight rose and fell with the pull on the strap, being not heavy enough to give a steady pull. Thus one machine was being driven as a motor by another equal machine, and was doing work which was measured by the friction brake. The pulley was 16 inches in diameter, and the speed 600 revolutions, giving 35,200 foot-pounds per minute, or more than 1 horse-power.

On putting on 28 lbs. on the pulley of the motor, the electro-motive force was reduced from 80 volts to 78 volts, and remained steady, and the machines still ran together. On putting 42 lbs. on the brake on the pulley of the motor, the electro-motive force was reduced to 76 volts, and then 56 lbs. was put on, when the motor continued to run at the same rate as the generator, and the electro-motive force was steady at 74 volts.

On account of the heating of the extemporised brake strap, it was not thought advisable to continue the experiments by adding heavier weights, although there seemed to be every probability that the machines would continue to run together at the same speed, the one as generator and the other as motor and doing work. The above experiments show that the work done on the brake was 140,000 foot-pounds per minute, or more than 4 horse-power.

In another experiment the three machines were started together, and run up to their usual speed of 600 revolutions per minute, each machine being driven by its own belt. The

belts were then thrown off two of the machines, and they were driven as motors by the current from the third machine, the electro-motive force at the terminals of the third machine being the same as when the three were running together in parallel circuit.

On November 10th, I again made further experiments with the three De Meritens magneto machines, and made measurements of the light given by the arc worked from the machines separately and combined in parallel circuit.

The resistance of the leads from the engine-room to the photometric gallery in which the measurements were made was ·032 ohms (cold), and the leads were heated considerably, and where exposed became quite warm to the touch when carrying the current from two machines or from the three machines.

One machine alone gave a current of 175 ampères, with an electro-motive force of 40 volts at the machine terminals, and an electro-motive force of 33 volts at the extremities of the arc, giving 5,775 watts expended in the arc and 1,225 watts expended on the leads.

Two machines worked well together, being driven independently, and gave an average current of about 275 ampères, with an electro-motive force of 47·5 volts at the machine terminals, and an electro-motive force of 35·5 volts at the extremities of the arc, giving 9,750 watts expended in the arc and 3,300 watts expended on the leads.

Another experiment with two machines gave an average current of 278 ampères, with an electro-motive force of 48 volts at the machine terminals, and an electro-motive force of 36 volts at the extremities of the arc, giving 10,000 watts as the energy per second expended in the arc.

These experiments give for

(1) One machine—

7,000 watts (electrical energy in external circuit);
1,715 watts (electrical energy in internal circuit);

giving 8,715 watts as the total electrical energy per second.

The power absorbed, as shown by indicator diagrams, during

this running was 13·8 horse-power, or 10,295 watts, giving an electrical efficiency of 84 per cent.

(2) Two machines—

13,300 watts in the external circuit;
1,800 watts in internal heating of coils;

giving 15,130 watts as the total electrical energy per second.

The power absorbed was 28·53 horse-power, or 21,280 watts, giving an electrical efficiency of 71 per cent.

The electro-motive force and current remained as steady as when the two machines had been mechanically connected together by clutches, completely proving that with equal machines such mechanical connections are entirely unnecessary.

This we had also found to be the case in the experiments of October 18th, when experimenting with two machines, and also with the three machines running on the same parallel circuit. The third machine has no mechanical connection with the other two machines, except through the belting, being driven from the same countershaft. When run together, the three machines gave a steady current of 300 ampères through an arc with an electro-motive force of 49 volts at the machine terminals, and of 37 volts at the extremities of the arc, giving 11,100 watts expended in the arc.

On account of the high resistance and consequent heating of parts of the lamp, and the irregularity of burning of the 40 millimètre carbons employed, the current from the three machines was only about 25 to 30 ampères greater than the current from the two machines.

The heating of the lamps was however greatly diminished by means of a by-pass, suggested by Dr. Hopkinson, from the conducting leads to the lower carbon, whereby a very considerable portion of the current was shunted past the lamp.

In the experiments on November 10th, with the three machines driven independently in parallel circuit, there was considerable variation in the mean electro-motive force and in the mean current. The irregularity was due to the very unequal burning of the carbons, which was noticed at the time of the experiments,

and not to any irregularity in the working of the three machines; for as the electro-motive force increased the current diminished, and there was comparatively little variation in the rate of work done in the arc. In one set of experiments the electro-motive force varied from 42 to 32 volts, and the current from 270 to 310 ampères, yet the product remained much more nearly constant, and the average was about 11,000 watts. In another set the electro-motive force varied from 41 to 37 volts, and the current from 290 to 316 ampères, giving an average of 11,600 watts in the arc.

The belt was now thrown off the third machine, which still continued at the same uniform speed, being driven as a motor by the current from the other two machines, with the arc still in the external circuit. Thus, of three alternate-current machines arranged in parallel circuit to supply current to an external circuit, one may be run as a motor by the current from the other two, which will continue to supply an arc in the external circuit. The electro-motive force at the extremities of the arc and the current through the arc were measured, and experienced wider variations than in the last experiment with three machines parallel, but the power in the arc remained very constant at about 12,000 watts, rather greater than when the three machines were driven directly from the engine. The arc also was observed to be steadier when the third machine was being driven as a motor, as though the motor acted as a governor on the circuit. The electro-motive force at the extremities of the arc varied from 52 to 41 volts, with occasional readings outside these limits, and the current varied from 240 to 285 ampères, the electro-motive force rising when the current diminished in such a way that the product remained very nearly constant. It would seem from these experiments that the work done in the arc with two machines combined in parallel circuit, without the third machine in the circuit at all, was considerably less than the work done when the two machines were supplying the arc, and at the same time driving the third machine as a motor.

Not only so, but when the machines were all three driven directly from the engine, the work done in the arc was rather less

than the work done when the two machines were supplying the arc and at the same time driving the third machine as a motor. These remarkable results are also borne out by the photometric results obtained during these experiments.

With the kind assistance of Mr. Longford and two of my students, Messrs. Anderson and Wordingham, I was able to measure the current, the electro-motive force at the terminals of the machine and at the terminals of the arc, as well as the candle-power throughout the experiments. The photometer employed was Dr. Hopkinson's dispersion photometer, in combination with a star disc and a paraffin lamp with a Methven slit. Ruby glass and ammonia sulphate of copper solution placed before the eye were employed to compare the intensities of red and blue in the two sources of light. By the Hopkinson photometer an illuminating power of 16,000 candles was reduced to six candles within a distance of 100 inches from the arc. The illuminating power given by the arc fed by the current from one machine was measured at a distance of 50 inches from the arc, and was found to be, with red light, 8,000 times, and with blue light, 16,000 times the light of the standard light.

At the same distance, the current from two machines gave 15,000 times the standard, as measured by red light. For these and higher candle-powers it was found convenient to remove the dispersion photometer to a distance of 100 inches from the arc.

The illuminating powers given by the current from two machines were then found to be 13,500 by red light, and 23,200 by blue light.

The illuminating powers of the current from three machines driven in parallel circuit were 16,000 by red light, and 31,000 by blue light; and when the two machines fed the arc and drove the third machine as a motor, the illuminating power was 17,300 as measured by red light.

Tabular Statement of Results.

	E.M.F. at the arc.	Current.	Watts in the arc.	Illumination.	
				By red light.	By blue light.
With one machine	33	175	5,775	8,000	16,000
With two machines ...	35·5	275	9,750	13,500	23,200
	36	278	10,000		
With three machines...	37	300	11,100		
	42 to 32	300	11,000	16,000	31,000
	41 to 37	310	11,600		
With two machines, driving the third as a motor	52 to 41	240 to 285	12,000	17,300	

A hearty vote of thanks was accorded to Dr. J. Hopkinson, and also to Professor W. Grylls Adams, for their communications.

The PRESIDENT resumed the chair and invited discussion.

Mr. ALEXANDER SIEMENS: My excuse for rising first in the discussion is that I wish to explain the table before you bearing the name of Messrs. Siemens Brothers. That table illustrates the facts which have been brought forward this evening by Dr. Hopkinson and Professor Adams; and I wish to say at once, clearly, that those experiments were made because we heard of the labours at the South Foreland, and there is no desire on our part whatever to detract from the merit of Dr. Hopkinson having first said that alternate-current machines could be used as motors. The figures now brought before you may be interesting.

The generator used, it will be observed, was a Siemens W^1 having 16 revolving bobbins, *i.e.*, 16 alternations for one revolution. The resistance of the bobbins was five ohms, and the speed 500 revolutions per minute. The machine used as a motor was a W^2 machine, which has only eight pairs of electro-magnets and eight revolving bobbins. It has therefore to run at just double the speed as the W^1, to get the same number of alternations per minute. Its internal resistance was 1·9 ohms.

Transmission of Power by Alternate Currents.

Generator. W_1, 16 bobbins in series, resistance of armature 5 ohms, speed 500 revolutions per minute.

Motor. W_3, 8 bobbins in series, resistance of armature 1·9 ohms, speed 1,000 revolutions per minute.

Generator.			Motor.	
H.P. absorbed.	Current in Ampères.	Resistance in Leads. Ohms.	Weight on Brake.	H.P. reclaimed.
·52	0	0	0	0
10·45	12·77	0	14 lbs.	5·33
"	"	3·8	"	"
* "	"	8·09	"	"
"	12·26	"	13 lbs.	4·49
"	"	12·06	"	"
* 12·2	"	14·79	"	"
"	11·55	"	12 lbs.	4·57
"	"	18·18	"	"
* "	"	21·74	"	"
"	10·81	"	11 lbs.	4·18
* "	"	27·5	"	"
11·2	10·01	"	10 lbs.	3·80
* "	"	30·0	"	"
10·45	9·14	"	9 lbs.	3·43
* "	"	36·2	"	"
"	8·43	"	8 lbs.	3·05
"	"	40·04	"	"
* "	"	45·0	"	"
"	7·9	"	7 lbs.	2·67
* "	"	48·8	"	"
"	"	"	6 lbs.	"
"	7·2	"	5 lbs.	1·90
"	"	52·5	"	"
8·35	7·1	55·5	5 lbs.	1·90
"	6·8	58·9	"	"
* "	"	65·0	"	"
7·31	6·14	"	4 lbs.	1·50
"	"	68·9	"	"
"	"	71·1	"	"
* "	"	"	3 lbs.	"
* "	5·8	"	2 lbs.	·76
4·5	9·6	3·8	"	"

* With these tests the motor kept running for some time, then it suddenly stopped—showing, therefore, the limit of brake-power with the resistance in circuit. The exciting current, which was the same in both machines, has not been taken into consideration in calculating the H.P. absorbed or reclaimed.

An important point in the transmission of power by alternate-current machines is the relation between the revolutions of the two machines employed. The number of revolutions of the motor is given by the number of alternations in the generator. In the present case the motor cannot run quicker or slower than double the speed of the generator. In our experiments some difficulty was experienced at first in starting the motor. We drove it as nearly as possible at the right speed, 1,000 revolutions, and then threw the belt off, but that was somewhat clumsy, and we succeeded in making the motor almost self-starting by starting the generator slowly, turning the motor by hand until it reached the right number of alternations with the generator, and then it increased itself in speed in accordance with the increased speed of the generator. The figures given in the tables are all *measured*, and you will see in a moment why I lay emphasis on that fact. The horse-power transmitted to the generator was measured by a Von Hefner dynamometer, which was put on the strap. The current was measured in the usual way by an electro-dynamometer. The resistance was made up by the standard resistance frames kept at the works. The power given out by the motor was measured by a Prony brake. Take the first item in the table. There was about 10·5 horse-power supplied to the generator; no resistance was put in the leads; the current was 12·77 ampères; a weight of 14 lbs. was put on the brake representing 5·9 horse-power from the motor. We left the same weight on the brake and increased the resistance until the motor stopped, which occurred when the resistance had been increased to 8 ohms. It ran for a while, but an accidental turn of the screw on the brake, or something, increased the load a little, which stopped the motor.

Observe that in the first case no resistance was in the leads, while in the third case there were 8 ohms. The horse-power taken up by the generator is in both cases exactly the same; the horse-power given out by the motor is in both cases exactly the same; and the current is the same in spite of the variation of resistance from 0 to 8 ohms. That the horse-power given out remains the same is not so very difficult of explanation, because the motor cannot turn any other number of revolutions than 1,000, because the alternations remain the same. The weight on

the brake is the same, so of course the horse-power given out is exactly the same. We proceeded with the experiments, decreasing the weight to 13 lbs. and increasing the resistance until the motor stopped, and so on until only 1·9 horse-power were given out, when the resistance in circuit was 48 ohms, 8·35 horse-power absorbed, and 7·2 ampères of current given.

In this case the current fell a little as the resistance was increased, until, with 65 ohms, the motor was stopped. At last ·76 horse-power only was given out. The current in that case was 5·8 ampères, the resistance in leads 71·1 ohms, and the horse-power absorbed by the generator was 7·31. We then tried what would happen if the resistances were decreased, and with 3·8 ohms resistance we had the same horse-power given out, ·76; the current increased to 9·6 ampères, and the horse-power absorbed fell to 4·5.

Now the only way in which I would endeavour to explain why the horse-power absorbed remains the same in the first three cases, although the resistance is varied, is that in the first case the surplus power is accelerating the motor, but the motor cannot go quicker than 1,000 revolutions per minute, so by the next impulse it is pulled back a bit, so to speak, and in reality, in the case where no resistance is in circuit, the motor is more oscillating than turning round smoothly at a uniform speed.

After those experiments were concluded, others were tried in the way Professor Adams has done at the South Foreland; but I am sorry to say that I cannot give the figures, because the experiments have only just been concluded, and have not yet been brought into a proper form; but I may say that we have connected a W[1] machine into two parallel circuits, and have then burnt 11 lamps parallel to the motor. But I will not continue to talk about these experiments, as they were only a repetition of what Professor Adams has done, and without figures the results cannot be shown clearly.

I am very much astonished that neither Professor Adams nor Dr. Hopkinson have mentioned one point. You all know that if a positive current is sent into a series-wound dynamo machine, the dynamo will turn, say, to the right. If the leads are reversed, and a negative current is allowed to pass into the dynamo, the dynamo

still turns in the same direction as when the positive current set it in motion. The idea therefore presents itself, that if alternate currents were sent into a dynamo it ought to turn. We tried that some years ago, but it would not do, and we gave it up until we tried the experiments I have referred to, when we thought of Professor Hughes' lectures last year, or the beginning of this, in which he showed us in how marvellously short a time one could magnetise and demagnetise bars. I thought that if Professor Hughes was able to do that so quickly, why should not an alternate current? So I had the magnet bars of a dynamo-machine constructed according to Professor Hughes' ideas, in order to make the changes of magnetism as easy as possible, and I am glad to say that on sending an alternate current through the machine it turned. The figures are not yet complete, or I had hoped to be able to bring them before you, but still the general experiments we made were very interesting. For instance, the alternate current sent into a series-wound dynamo machine turned it round. Then, to increase the effect, we thought there might be some advantage in getting the phases exactly alike in the helix and in the electro-magnet; by connecting them parallel the two resistances were almost equal, and we obtained a little bit better effect. Then we tried the machine again as a series machine, and shifted the brushes to see what that would do, and we found the very curious result, that however far we shifted the movable brushes, the machine always turned in the same direction; but if you held the machine forcibly, and then turned it the other way, it would continue to go in the latter direction. I am sorry that I cannot bring any figures on this matter before the Society, but I considered I was justified in mentioning these facts before the discussion on the papers began, because they seemed to have a direct bearing on the question.

I would just add that of course the dynamo machine starts entirely by itself.

Professor W. E. Ayrton: The communications we have had this evening are extremely interesting, because they form experimental corroborations of theoretical predictions made by Dr. Hopkinson a year ago, in his lecture given before the Institution of Civil Engineers. It is one thing to work out

mathematically an experiment when it has been made—a good many can do that; but it is a totally different thing to work out theoretically a result, and then find out afterwards that is is experimentally true. It is a totally different order of work, and that is what Dr. Hopkinson has done in this case.

It has of course been known for a long time that, when a motor like the old Wilde machine was driven by an alternate-current dynamo, synchronism was obtained, and there was perfect governing. I do not know how long ago that was known, but it was a long time; and Professor Forbes, in a communication made a year or so back to the Royal Society of Edinburgh, gave results of his experiments in driving motors by alternate-current dynamos, and drew attention to their marvellous synchronism. He did not develop the subject of driving motors nearly as fully as Dr. Hopkinson has done, and gave no indication, as far as I remember, that a motor having a higher electro-motive force than the generator could be driven as a motor. Still less did he or anybody else show, as far as I am aware, until Dr. Hopkinson showed us, that this synchronous governing of a motor by an alternate-current dynamo really furnished the solution of working two alternate-current generators. The very fact that a motor can be worked by an alternate-current dynamo, and gets the phase of its electro-motive force rendered almost opposite to the phase of the electro-motive force of the generator which enables it to work as a motor, shows that the two alternate-current generators cannot work in series; and next, as Dr. Hopkinson has pointed out, shows also that they can work in parallel.

The formulæ that Dr. Hopkinson has used in this most interesting paper are based on those that he has himself given in his article in the "Encyclopædia Britannica," and are based on the original formulæ given by Joubert, in his paper on alternate current machines, in the "Annales Scientifiques de l'École Normale Supérieure."

In all these formulæ the electro-motive force is given as a sine function of the time, and quite independent of the current produced by the dynamo. In fact, the electro-motive force is merely a sine function, and would appear to be the same, from

these formulæ, whether the alternate-current dynamo is producing a large or a small current. But this seems rather improbable.

You know the original curve given by Deprez some time back for a continuous-current machine, being obtained from experiments made with separately-excited machines, was not really the curve that is obtained connecting electro-motive force and current for a series dynamo, because it was based on an experiment made with small current always in the armature; in fact, it was the curve which would connect the electro-motive force of the machine with the current round the field-magnets, when no current was flowing round the armatures. Subsequent experiments showed that the original so-called characteristic curve, instead of continuously going up, comes down again towards the end; and this coming down is very marked in, say, a Brush machine, where the armature is a powerful magnet relatively to the field-magnet; and also, as Deprez subsequently pointed out, that in a separately-excited dynamo, instead of giving a constant electro-motive force independent of the current, the electro-motive force falls off very much in such a separately-excited dynamo, as the current round the armature of the separately-excited dynamo is allowed to increase by a diminution of the external resistance. In fact, the curve is something like the drawing I now make on the black-board. I do not attach any particular importance to the exact shape I have here given to the curve. As a matter of fact, the subject of the connection between the electro-motive force and the current in separately-excited dynamos, of which an alternate-current machine is a type, has been receiving the attention of my colleague and myself for a long time, and we may have an opportunity of going into the matter later on, and be able to show how the electro-motive force does fall off, due to the weakening of the field-magnet field by the current field set up by the armature; and which weakening of the magnetic field by the current in the armature has not been taken into account in the formulæ hitherto employed for alternate-current dynamo machines.

Now, experience shows that the very experiments that Dr. Hopkinson has referred to, viz., the greater heating of the armature when revolving on open circuit, and the greater power

necessary to turn the armature on open circuit when no current is passing, are a proof that the total magnetic field is stronger when the machine is turned on open circuit, and there is no current in the armature, than when it tends to weaken the field, and so cause the electro-motive force to fall off. This heating leads to the fact that a falling-off in the field is produced by the current in the armature; and therefore, in the formulæ that have been employed, instead of using the expression

$$\mathrm{E} \sin. \frac{2 \pi t}{\mathrm{T}}, \text{ etc.,}$$

where E is a constant for a constant speed, and independent of the strength of the current, x, in the armature of the alternate-current machine, it seems to us that where there is no iron in the armature coils this expression must be replaced by something like

$$(\mathrm{E} - p\, x) \sin. \frac{2 \pi t}{\mathrm{T}}, \text{ etc.,}$$

and where there is iron, by a still more complicated expression,

$$\frac{\mathrm{A} - p\, x}{1 - \mathrm{S}\,(\mathrm{A} - p\, x)} \sin. \frac{2 \pi t}{\mathrm{T}}, \text{ etc.,}$$

to allow for the magnetic saturation of these iron cores.

We are not yet in a position to speak of all the changes that varying this constant will introduce, for, to speak candidly, the mathematical development is not completed; but it seems desirable that this correction, to allow for the weakening of the magnetic field due to the currents of the armature, should be taken into account.

I would ask Dr. Hopkinson whether my memory is right that Joubert made the curve of potential something of this kind in an arc lamp worked with alternate currents:—steep at first, and then more gradually curving to nought, and not steep both at its commencement and at its termination, as Dr. Hopkinson has drawn it?

Dr. J. Hopkinson: Yes; you are right.

Professor W. E. Ayrton: Another point in the paper is extremely interesting, viz., that part about the measurement of these alternate currents.

In a paper recently brought before this Society, on Measuring

Instruments, attention was drawn to the fact that a Siemens dynamometer, when used for alternate currents, measured the square root of the mean square of the current, which is not of course the same thing as the mean current. When the equation of electro-motive force is a sine function of the time, and therefore the equation for current is also a sine function of the time, then the mean current is 0·9 of the square root of the mean square. But in the particular case considered by Dr. J. Hopkinson, at the middle of page 507, where the function is a combination of a sine function, a straight line inclined to the axis of time, and a straight line parallel to the axis of time, the result is quite different; and he comes to the conclusion that the square root of the mean square is one-sixth too much, and that the mean current in that case would be six-sevenths of what the electro-dynamometer would measure, and which in all cases is the square root of the mean square. Dr. Hopkinson has drawn attention to the proper way in which to measure the work done by an alternate-current dynamo, which cannot be measured by means of even a quadrant electrometer, if that instrument only is used to measure the difference of potentials, while an electro-dynamometer is used to measure the current. I am sorry to say that I must confess that on certain occasions, as at the Crystal Palace Electrical Exhibition, for instance, where it was desired to approximate the power put into arc lamps worked with alternate currents, my colleague and myself were compelled to use a quadrant electrometer to measure the difference of potential between the carbons, and a Siemens electro-dynamometer. We so obtained an approximation to the power, but of course the result was but an approximation.

As Dr. Hopkinson has said this evening, there is only one way of measuring the power given to a portion of a reversed-current circuit containing resistance and an electro-motive force, and that is by a method which I see Mr. Gray in his recent excellent little book attributes to M. Potier, I suppose because M. Potier published it in October, 1881, in *Le Journal de Physique*, Vol. X., but the method, as a matter of fact, was originally arrived at by Professor Fitzgerald of Trinity College, Dublin, and myself, independently of each other, at one of the early meetings of the

Electrical Conference at Paris in August, and was communicated at the time by myself to Sir William Thomson. The difficulty, however, of employing this proper method, which requires the use of a quadrant electrometer only for measuring both currents and potential difference, and which is given in detail in a paper by Professor Perry and myself, p. 272, Vol. XI. of the Journal of this Society, arises partly from the sensibility of a Thomson's quadrant electrometer becoming small unless the charge in the needle is tolerably great, and partly from the diminution of the current in the main circuit, produced by the introduction of the auxiliary resistance. It was indeed partly this want of sensibility of the electrometer in making this particular test that led us to take up our multi-reflex principle for greatly magnifying the indications of an electrometer.

The subject of a condenser in connection with an alternate-current dynamo is to me a subject of great interest. The suggestion of using such a condenser is, I believe, due to Mr. Munro; at any rate, I remember that at the end of 1878 or beginning of 1879 that gentlemen took out a patent for such a combination, and Mr. Latimer Clark, who had an interest in the matter, and at whose works the experiments were conducted, asked me to make experiments on this very subject. Without working out the details, and getting the exact difference of potential, as Dr. Hopkinson has done, I suspected that the electro-motive force would be rather great, and therefore, although the numerous condensers at Messrs. Clark & Muirhead's works were at my disposal, I did not like to use them, and I made a condenser of my own, but I could not get it to stand the high electro-motive force. Mainly on this account I abandoned the experiment, and advised Mr. Munro not to expend money to complete his patent for combining a condenser with alternate-current circuits.

The use of alternating currents to work an ordinary dynamo-machine as a motor has been referred to by Mr. Alexander Siemens, who has pointed out that the arrangement is not very satisfactory.

Mr. ALEXANDER SIEMENS: I have not said that the arrangement was unsatisfactory. I said that when we tried it years ago it was

perfectly satisfactory, but that figures referring to recent experiments were not yet completed.

Professor AYRTON: I thought that was implied from the fact that the dynamo motor would go indifferently in either direction, and therefore I presume it has no great power when going in either.

Experiments were made by Professor Perry and myself, during the Electric Exhibition at the Aquarium, on Messrs. Goulard and Gibbs' alternate-current circuit with motors originally made to work with continuous currents, and not very good results were obtained with alternate currents, as the power of the motor was very small.

It has, however, to be noticed that, when motors intended to be worked with continuous currents are worked with alternate currents, there is no automatically produced synchronism of the generator and motor, such as occurs when the motor will only work with reverse currents.

As to getting sparks from a Ruhmkorff coil, which Dr. Hopkinson has referred to, that is a very interesting aspect, because it shows that, if in an alternating-current circuit which is perfectly insulated from the ground, both dynamo and wires, one of the wires is touched, a shock is obtained the repetition of which is not desired. It is usually assumed, and was assumed in a recent report, published in the *Electrical Review*, of some men who were killed through touching wires through which alternate currents were passing, that the wire or wires must have been touched in two places to kill the men. As a popular experiment, I some time ago arranged a Ruhmkorff coil, with its battery, all on a stand perfectly insulated from the ground by its standing on glass rods, which were dried artificially with sulphuric acid, so I had the whole apparatus perfectly insulated; but as one would expect, distinct shocks were obtained from touching any part of the circuit in which the alternating currents were flowing, leading to the conclusion that a perfectly insulated alternating-current circuit gave shocks if touched in one place. Also, if a person be insulated from the ground, sparks can be obtained by his insulated body if he approaches it to the insulated wire, in consequence of

the potential of his body undergoing rapid periodic variations, and the continuous passing of those sparks in and out of him will be decidedly unpleasant.

I may end these few remarks by congratulating Dr. J. Hopkinson on this most successful application that he has made of theoretical considerations to what may almost be called an industrial fact, the coupling of two alternate-current dynamos, such an application being of course the essence of all technical progress.

Dr. EDWARD HOPKINSON: In the remarks made by Mr. Alexander Siemens, he referred to the table which he has prepared, and which illustrates very concisely some of the definite results given in the two papers. The table also shows the need of a warning contained in the paper first read. Let us compare, for example, the first and third row of figures. The speeds and the power absorbed and transmitted by the generator and motor remain constant, the current also is said to remain constant, but the resistance in circuit is increased by 8 ohms. This of course is a very paradoxical result, for what becomes of the power in the first case, which is spent in heating the extra resistance in circuit in the second case? Clearly the current has been measured by an electro-dynamometer, and the value given in the table is not the true value, and, as shown in the paper, the true value must be considerably less when the resistance is increased. As the resistance of the leads is increased, the difference in phase of the electro-motive force and of the current also increases, and the error in the use of the electro-dynamometer to determine the current increases. The effect of the lagging of the phase of the current behind that of the electro-motive force is very well illustrated in the secondary generators which have been worked out by Goulard and Gibbs. There, when the load on the line is increased, the generator is maintained at the same speed, but the electro-motive force increases, and the relative phase of the electro-motive force and current changes, and therefore the lamps upon the secondary circuits can be lit up with just the same ease as when there is a smaller load on the line. This can only go on, as shown clearly by Mr. Alexander Siemens, for a certain time: limits of discontinuity will arise. The couple on the motor shown on

the table is maintained constant for the first three readings; then the machine suddenly stops, as indicated by the *. To allow the machine to start again, the couple, *i.e.*, the load upon the brake, has to be diminished.

The table also illustrates the way in which alternating-current motors cannot be governed. It shows that varying the resistance makes no difference to the speed. The paper read by Dr. John Hopkinson indicates clearly how they may be governed in an entirely different manner.

We have had to-night examples of theory going ahead of practice in experimental work. I think that Mr. Alexander Siemens' remarks have given an example of experimental work going ahead of theory, and I hope he will continue his most interesting experiments, especially with regard to the use of dynamos in connection with alternate-current machines.

Mr. H. E. Harrison: We certainly owe our best thanks to Dr. Hopkinson, not only for the problems in connection with alternating-current dynamo machines he has solved, but also for the solution of many other questions that his treatment of the subject will enable us to answer.

There is, however, an expression in the paper which seems to call for a little explanation, viz., the expression $E \sin. \frac{2\pi}{T}(t + \tau)$ for the electro-motive force of the machine at any instant.

According to Fourier's theorem, any curve representing a periodic function can be built up, as it were, of a series of curves of sines. But the expression used by Dr. Hopkinson gives the simplest possible of such curves—that is, the simple curve of sines itself.

Considering the widely differing forms and dimensions given to dynamos by different makers, it would certainly be most remarkable if the curves showing the variations of electro-motive force were all of precisely the same general form, and that, too, the very simplest they could assume.

As a matter of fact, some time back I tried some rough-and-ready experiments with a Siemens alternating machine. They were not accurate, as the apparatus was hastily made, and came

to grief once or twice in the middle of determinations. But laying off the results, such as they were, as curves, and drawing on the same sheet of paper curves of sines whose maxima and minima coincided with those of the experimental curve, I found in every case that the curve of sines lay wholly within the latter.

Professor Foster, to whom I spoke of my results, was kind enough to have traced for me certain combinations of curves of sines, by means of a machine he possesses for the purpose.

One of these looks suspiciously like my experimental curves; if I recollect rightly, it was compounded of two curves whose periods were 1 to 3, amplitudes 10 or 12 to 1.

So that if my experiments were not too rough to be worth anything with a Siemens machine, at all events the electromotive force at any instant must be expressed as the sum of two such expressions as the one given by Dr. Hopkinson.

Professor AYRTON: May I ask whether you used an electrometer?

Mr. H. E. HARRISON: No. The machine was open-circuited, and I used a commutator with four teeth in conjunction with a sensitive astatic galvanometer.

Professor SILVANUS P. THOMPSON: A good deal has been said about the defects of an electro-dynamometer for measuring an alternate current. There is one defect, however, that I think has not been alluded to, and that is the failure of the electrodynamometer to give reliable indications when made in such a form as to have one or both coils with very many convolutions. We have that defect in the case of the so-called watt-meter, where one of the two coils acts as the potential part of the arrangement. There the great number of convolutions have the effect of offering, not only resistance, but considerable self-induction to the current; and as a consequence, the indications given by the instrument when used with alternating currents, not only do not agree with the indications given when used for steady currents, but the reading will be different when the current is one of rapid periods of alternation from that given when the alternations are slow. The more rapid the alternations the greater is the effect of the self-induction in proportion to the

resistance. That, I think, is fatal to the use of the watt-meter. Not only can you not rely on the calibrations made by a steady current, but neither can you rely on readings made with one set of alternate currents corresponding with readings made with another set of alternate currents. Two equal readings made with two alternating currents of different periods will not in general mean an equal number of watts.

I would ask Dr. Hopkinson whether a mistake has not occurred on the last page of the proof of his paper, where the expression $\frac{2\,\pi\,\gamma}{L}$ is squared. Is there not a mistake in putting down the "square"?

Dr. J. HOPKINSON: I think not.

Professor S. P. THOMPSON: Then how does it happen that, the coefficient of self-induction being a quantity in the nature of a line, a length, that coefficient when divided by a time has to be squared?

Dr. J. HOPKINSON: You are quite right there; the resistance in ohms should be "squared" to correspond.

Professor S. P. THOMPSON: I do not ask the question in a cavilling spirit, but because I have myself been taken to task when following the example set by M. Cabanellas and by Professors Ayrton and Perry, viz., to express as a resistance the effect arising out of self-induction, and which, because it is of the nature of a velocity, may rightly be expressed in "ohms."

In regard to the question concerning the possible variation of the coefficient of mutual induction between the field-magnets of a separately-excited machine and the armature, that variation no doubt does exist. If the term which expresses this fact be introduced into the equations it will aid us to account for some of the still outstanding discrepancies that have been observed. I would like to point out that I have myself referred to the matter in one of the appendices of my book on dynamo machines. Mr. Moorsom, who has worked with me at some of the mathematical problems of dynamos, pointed out that there was one term wanting in the differential equations of the dynamo machines, as written by Joubert and others, and, although that absent term is held by

some to be really an unnecessary term altogether, I think that it ought not to be omitted, since it will, if properly interpreted, account for the discrepancies arising from this effect of the reaction of the armature upon the field-magnets. It may be negligible in most of the modern continuous-current dynamos, but it certainly cannot be negligible in those continuous-current dynamos where the armatnre is very powerful in proportion to the field magnets; and I do not think it is at all negligible in those alternate-current machines where there is iron in the cores of the armatures.

It was stated by Mr. Alexander Siemens that he had made an alternate-current machine into a motor which ran either way, and would run in almost any position of the brushes. I have observed the very same thing with a direct-current motor, but it was one which had very badly proportioned pole-pieces to its field-magnets, and in which also the armature was very much bigger than the field-magnets. The armature magnetised a portion of the field-magnets on ahead of itself, and then attracted itself up to it. No matter which way it was started, or where the brushes were, it would run as a motor, and it sparked in every position of the brushes.

Mr. A. Siemens: I forgot to say that when we used the dynamo as a motor we also tried sending the current through the helix alone, and not round the electro-magnets, which were entirely disconnected. The machine turned also in this case, but apparently with less power than before.

Professor S. P. Thompson: With respect to the combination of a circuit in which there is an alternate-current machine with a condenser, I may perhaps be permitted to remind Professor Ayrton that the use of a condenser in conjunction with a circuit of that kind goes back a little earlier than he thought. I remember, at the Paris Exhibition of 1878, seeing in the little shed erected outside the main building, where the Jablochkoff system was being displayed, an enormous pile of condensers which Jablochkoff was applying to his system; but they were subsequently abandoned, and for very good reasons. This very enormous apparent increase in the electro-motive force when a condenser (and nothing else) is put on to an alternate-current machine really does not increase the effectiveness of that machine

for any useful purpose. It is virtually the same as taking a continuous-current shunt-wound machine on open circuit, putting on a voltmeter, and saying that it gives an enormous electro-motive force, and will therefore give so much force running through lamps. I do not doubt that the enormous force of 8,000 volts is obtained by the condenser; but directly you begin to enquire how much electricity is running into the condenser at each charge, and out again, you will see that this depends upon the capacity of the condenser as well as the electro-motive force. It must be remembered how little electricity condensers will contain compared with that expended every second in an arc or incandescent lamp; and it cannot be expected that the addition to the mains of a condenser of any ordinary capacity will increase the available current in any useful way.

Dr. J. Hopkinson: My suggestion was that the arrangement would be useful for destroying condensers.

Professor S. P. Thompson: I am extremely glad to have elicited that point, because I was rather afraid that the other conclusion might be drawn from the apparent increase of electro-motive force.

Lastly, as to the peculiar effect to be got out of an induction coil, sparks can be taken by an insulated person from either terminal. Dr. Hopkinson has pointed out a cure for a badly-constructed coil, by putting a sheet of tinfoil on the outside. The converse effect can be produced even on a coil properly constructed. Connect one terminal of the secondary circuit to any conductor having a large surface, or to the earth, you will then be able to get sparks from the other terminal, and from that one only. But sparks can be drawn from all neighbouring conductors, by reason of the static effects of the alternating currents in the coil. I investigated this matter, and in a few observations of mine, published in the *Philosophical Magazine*, I showed, amongst other things, how the alternating character of the spark might be investigated by sending it through a small Geissler tube, and then viewing the images in a rotating mirror.

The President suggested the adjournment of the discussion until the next meeting on November 27th, 1884, and the meeting was adjourned accordingly.

The One Hundred and Thirty-eighth Ordinary General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 27th November, 1884—Professor W. GRYLLS ADAMS, F.R.S., President, in the Chair.

The minutes of the previous meeting were read and confirmed.

The following transfers were announced from the class of Associates to that of Members:—

Alfred Richard Sennett.
Henry Starke.
Albert A. L. Straube.

The PRESIDENT: By the kindness of the Eastern Telegraph Company, through Mr. W. T. Ansell, one of our Members of Council, a telegram was sent, after our last meeting, to Mr Edward Davy, in Australia, to announce to him the fact that he had been elected an Honorary Member of the Society; and I am glad to say that through the courtesy of the same medium the following telegraphic reply was received from Mr. Davy:—"Please convey to the Society of Telegraph-Engineers my cordial thanks for the honour conferred upon me, which I cannot fail to appreciate highly." We were very glad to have this rapid means of communicating with Mr. Davy, and I am sure that the Society will be very thankful to Mr. Ansell and those who have facilitated that object.

The following donations to the Library were announced, and the thanks of the meeting awarded to the donors:—Mr. H. Fontaine, the International Health Exhibition, the Institution of Civil Engineers, the Institution of Patent Agents, the Astronomer-Royal, and eight volumes of evidence in the case The American Bell Telephone Co. *v.* The People's Telephone Co., probably from J. J. Storrow, of America.

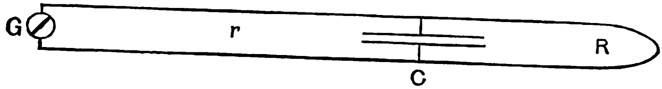
The PRESIDENT: We now come to the business of the evening, which is the adjourned discussion on the two papers read at the last meeting, on Alternate-current Machines, but before that discussion is resumed, Dr. Hopkinson would like to add a note to his previous communication.

Dr. HOPKINSON: The note which I desire to add to my paper is the following:—My attention has only to-day been called to a paper by Mr. H. Wilde, published by the Literary and Philosophical Society of Manchester, 15th December, 1868, also *Philosophical Magazine*, January, 1869. Mr. Wilde fully describes observations of the synchronising control between two or more alternate-current machines connected together, and explains it so far as was possible with the public knowledge of the time. I am sorry I did not know of his observations when I lectured before the Institution of Civil Engineers, that I might have given him the honour which was his due. If his paper had been known to those who have lately been working to produce large alternate-current machines, it would have saved them both labour and money.

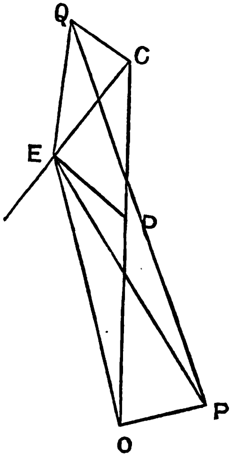
The PRESIDENT: I hope that members, after considering the papers that have been read and the remarks that have been made in the early part of the discussion, have come prepared to carry on the discussion and to throw further light on this interesting subject, the working of two or more alternate-current machines on the same circuit. I hope that some members may have some account to give of experiments which have possibly been made in the interval since the last meeting.

Mr. THOMAS H. BLAKESLEY: Allow me, Sir, to say a few words upon this interesting subject, which has been so extremely well worked out by Dr. Hopkinson, and in the first place to say that in presenting this paper he has marked a distinct step in mechanics. I say mechanics, because I think that when these electrical matters are better understood there will be no distinction between electricity and mechanics. The paper itself is remarkably exact in every way, but I venture to think that a different mode of treatment of the subject would have rendered it rather clearer to some of us; and I rather wonder that Dr. Hopkinson has not adopted the geometrical method of exhibiting some of the results which he has brought out in his paper. By an illustration on the board I will, with your permission, show how one or two of his points and others can be with remarkable simplicity represented by the means of a geometrical diagram.

I will, as an example, take Section VII. of Dr. Hopkinson's paper, in which he refers to the effect of a condenser in connection with a circuit; but I will make it a little more general than he has done, as it will be easy enough to take the more limited case



afterwards. Suppose G an alternating generator working into a circuit whose total resistance is $\overline{R + r}$, and that C is a condenser of capacity C, connected with the circuit in such a way that the section of the circuit terminated by the plates of the condenser, and containing the generator, has a resistance r, and the remote section has a resistance R. If R is infinite, we have the case contemplated by Dr. Hopkinson.



Take a straight line, C O, to represent the electro-motive force of the generator when at its greatest. Divide C O in D, so that C D : D O : : R : r.

Calculate the quantity $\frac{C \pi R r}{T(R+r)}$ where T is half the complete period of alternation. This magnitude is numerical, and may therefore be represented by the tangent of an angle. Set off

O C E, an angle whose tangent has the above value. From D draw D E perpendicular to C E, and join E O. Then C E, E O represent in *phase* and *magnitude* the maxima values of the effective electro-motive forces in the sections R and r respectively. It is easy to see that if the angle O C E has any magnitude, E O must be greater than D O, which would have been the effective electro-motive force in r if there had been no condenser. This is what Dr. Muirhead pointed out to Dr. Hopkinson; but as to the other section, R, the line E C must be less than C D, therefore here the effective electro-motive force is less than what would have been the case had there been no condenser.*

If, besides the mere resistances, there exists self-induction in one or both sections, the diagram can easily be modified to represent the case. It is only necessary to form the function appearing so often in Dr. Hopkinson's calculations, $\frac{L}{T}\frac{\pi}{r}$, where L is the coefficient of self-induction for the section under consideration, and r is its resistance. It is numerical. From E set off angles whose tangents are equal to the quantities so calculated from E O, E C respectively, and in the direction of advancement, the rotation of the diagram being here supposed contrary to that of the hands of a watch. From O and C lines must be drawn perpendicular to E O, E C respectively, to cut the lines so formed. The points of intersection being then joined, the line between them will represent in phase and magnitude the electro-motive force of the generator necessary to produce the observed effects. Let the points be P Q respectively. Here I should like to point out that since O P is at right angles to E O, Q P may be less than Q O. Thus the effect of self-induction in the section r is, up to a certain point, when Q P is perpendicular to O P, beneficial—that is to say, its presence enables a smaller impressed electro-motive force to produce a given effect than would have been necessary had there been no self-induction in that section, which is not the general opinion on self-induction.

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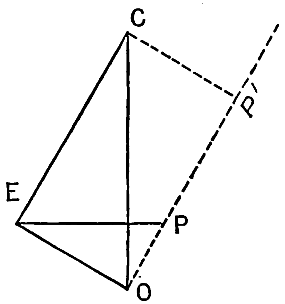
* This diagram and the following ones are supposed to undergo uniform rotation through the four right angles in one complete period. The electro-motive force at any point of time in any case is the projection of the line representing the maximum value upon some *fixed* line.—T. H. Blakesley.

I will now take the case where R is infinite.

The tangent of the angle O C E is now $\frac{C \pi r}{T}$. O E is drawn perpendicular to C E.

The tangent of the angle O E P is $\frac{L \pi}{T r}$, and O P is perpendicular to O E, and therefore parallel to C E. C P is the impressed electro-motive force.

The angle P E O may be so great that C P is perpendicular to O P, as shown by C P′. In this case the electro-motive force impressed is the minimum to produce the observed effect due to E O, the effective electro-motive force. In this case, too, of course the two are equal and in the same phase, and the effect of the condenser has been to annihilate the effect of self-induction and the



breach of continuity in the conductor caused by the condenser itself, the effect in current being the same as would have resulted from the same electro-motive force acting upon a *continuous* conductor devoid of self-induction and without a condenser. This remarkable state of things takes place when $\frac{L \pi}{T r} = \frac{T}{C \pi r}$, or $C L = \frac{T^2}{\pi^2}$, and is therefore *independent of the resistance.*

This mode of representing the electro-motive forces enables one very readily to perceive the difference of phase, because all the lines are in their proper phase with respect to all the others. C E in the last figure represents, for instance, in phase and magnitude, what Dr. Hopkinson has called the amplitude of oscillation of potential between the two sides of the condenser,

and this potential difference comes to a maximum at a quarter of a period after the current in the conductor does, represented by the right angle C E O.

So far I have been dealing with a condenser at a point, a local condenser, but we may have a distributed condenser—a cable, for instance, possessing greater capacity the greater its length. I do not know that the interesting problem of such a conductor subjected to an alternating current can be readily represented by a diagram, because there is a continual change of phase as each point of the conductor is considered, but it is easy to understand from the diagrams I have used that the currents in places near the generator are in an advanced phase compared to those more remote. This effect will have a great bearing, I fancy, on the transmission of power by alternate currents. I have worked out some of these cases, and find that the capacity is likely greatly to influence the efficiency. Without going into the analysis, I may give you the following interesting result:—

Suppose ρ is the resistance in ohms of a quadrant of the cable, K the capacity in farads of a quadrant of the cable, form the quantity $\left(\frac{\rho\, K\, \pi}{2\, T}\right)^{\frac{1}{2}}$, which call a, a quantity whose dimensions are the inverse of linear space. If now the distance of transmission by such a conductor is l, and there is a return main to the dynamo, then the dynamometer readings near the dynamo and at the distant point will be to one another in the relation represented by the arithmetical mean of the hyperbolic and circular cosines of $2\, a\, l$.

$$\frac{\text{Cosh. } 2\, a\, l + \cos.\ 2\, a\, l.}{2}$$

From data given by Mr. Latimer Clark on a certain Hooper's cable provided for crossing rivers in India, I find that with alternations at 200 per second we should have the dynamometer readings reduced to one-half, if the distance were 1·8 kilometers, with such a conductor. What is more, the dynamometer readings being as the squares of the currents, much more heat is generated in the near sections of the conductor than further away, and I suggest that this may be one of the causes of the heating of

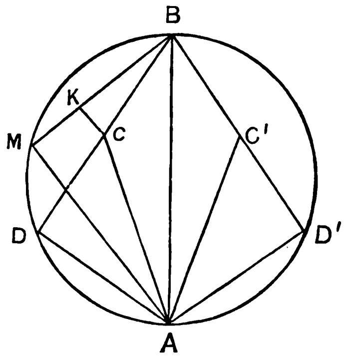
alternate-current machines, though I am not prepared to say that this effect would be excessive. Undoubtedly the tendency is in that direction.

I should like to add the diagram of the transmission of power problem.

Let A C, A B represent the electro-motive force of the follower and driver, in phase and magnitude.

The positive direction of A C is the opposite to that of A B.

Then, in the diagram, A C must be drawn in advance of A B. Join B C and produce it, and let A D be drawn from A perpendicular to B C.



Then B D, D C, C B are proportional respectively to the power exerted by the machine generating the electro-motive force A B, the power transmitted to machine generating A C, and the power going to heat the circuit. The absolute values of these three powers are obtained by multiplying the above lines by B C and dividing by twice the resistance.

It is clear that if D C has a negative value, the follower does work. For true transmission under such circumstances C must lie within a semicircle upon B A, for B D A is a right angle, and D therefore upon the circumference. If the diagram so far is reproduced on the right side of the line A B, with dashes for the corresponding letters, it may be shown that the angle C A C′ represents the limit to which C A may be displaced without upsetting the stability of the motion. If, therefore, C A approach

B A in value, the angle C A C′ becomes smaller, for C must lie within the circle: so that in practice it would be a good thing not to have the two electro-motive forces very nearly equal to one another.

If there be self-induction in the circuit, which in practice would be largely the case, the necessary change in the diagram is to draw B M at such an angle to B C, in the direction of retardation, that its tangent is equal to $\frac{L\,\pi}{T\,r}$, L being the coefficient of self-induction and r the resistance, and then to drop the perpendiculars C K, A M upon this line.

Then the lines B M, M K, K B are proportional respectively to the power exerted by the machine generating A B, the power transmitted to the machine generating A C, and the power going to heat the circuit. The absolute values of these powers are obtained by multiplying the above lines by B K and dividing by twice the resistance.

I am much obliged to you, Mr. President and gentlemen, for the kind attention you have given to my diagrams.

Mr. Gisbert Kapp: I have no remarks to make, but would like to ask a question of which I have myself been unable to find a solution. We have heard of "power being lost," and I would ask, Where does it go? I have made experiments with alternate currents on a solenoid for the purpose of working an electric governor. The object in such a case is of course to obtain a maximum of pull on the iron core, and I find that the resistance in a solenoid must be very low indeed in order to get a good current through and to have a good pull. I have not the figures by me, but, approximately, the solenoid had a resistance of 3 ohms, and an alternate-current machine was used, the potential of which was kept constant at 100 volts by varying the existing current. When the iron core was not in the solenoid, the alternating current, measured by a Siemens dynamometer, was 10 ampères. I have found that in this solenoid there was a counter electro-motive force of 70 volts, inasmuch as 30 volts would be sufficient to get a continuous current of 10 ampères through 3 ohms. The potential was measured by a Swan lamp. This was

first standardised by a continuous current, and a curve was drawn out which gave the potential and candle-power, and then when the lamp was put on parallel to the solenoid the candle-power was taken, and the potential read off the curve.

By varying the exciting current through the Siemens field-coils it was possible to get the potential afterwards very near 100 volts, so that any error in the reading of the curve must have been very slight. Thus we have a potential of 100 volts and a current of 10 ampères, giving, if you multiply the two (I believe it is not correct to do so, but it may be admitted in a rough calculation), 1,000 watts, whilst with 300 watts you would have obtained the same current through the solenoid with a continuous current. 700 watts have gone somewhere. The solenoid became heated a little more than with a continuous current; I cannot say how much more, but I do not think it so much that 700 watts can be accounted for. Perhaps Dr. Hopkinson will be able to tell us something on that point.

Mr. J. N. Shoolbred: I would venture to make just a remark or two upon these very valuable papers. At the very outset I must confess that my experience with alternate-current machines is very small, for the reason that I have always looked upon that kind of machine as presenting very serious drawbacks, which in general application made them inferior to continuous-current machines. One of the drawbacks is that the employment of alternate-current machines means the sacrifice of the use of storage, while the other drawback means the sacrifice of the transmission of power. The latter stigma, as it may be called, is partly removed, it may be hoped, by the very valuable papers we have had from you, Sir, and from Dr. Hopkinson. It must be the general wish that such a removal will be effected, for if so it will mark a very valuable era in these otherwise useful machines.

A further drawback in the employment of these machines is the undoubted increased danger from the shocks that are received from them. The electro-motive force generally used in alternate-current machines is very serious, compared with that of continuous-current machines, and that fact causes hesitation in their use. The reason that I ventured to make a remark or two upon the

matter was, that on the last occasion Professor Ayrton, I believe, mentioned that with alternate-current machines contact with a single wire would be sufficient to produce a very serious shock. That remark brought to my mind a little experience I had in this direction a short time ago, while experimenting with the secondary generators of Messrs. Goulard and Gibbs, which have been a good deal spoken of, and which were in use on the Metropolitan Railway. I happened at the Edgware Road Station installation to accidentally touch one of the terminals, and, not being myself insulated from the ground (not being on some small india-rubber mats which were there placed), I received a very violent shock. That incident, I think, directly contradicted the statement of Messrs Goulard and Gibbs in their recommendations of the instrument, and rendered it pretty evident that machines of that kind were not safe to place in cellars or other places attached to private houses, where, though they might at first have been properly insulated, yet after a length of time would become a source of danger to the inmates of the house. That fact in itself, I would venture to think, constituted a very serious danger, and one that would form a considerable bar to their application largely.

The main feature of the papers under consideration this evening is undoubtedly the use of the transmission of power thereby obtained from these machines; and the part which you yourself, Sir, have alluded to, especially the experiments that are going on at the South Foreland, was particularly interesting, in consequence of the largeness of the currents used there.

We venture to hope that later on, perhaps, these experiments at the South Foreland, when completed (I understand that they will be continued to the end of the year), may be laid before the Institution, and then probably these very valuable theories that have been shadowed out, and the experiments which seem to have previously been carried out by Mr. Wilde, will be more fully explained; and the thanks of this meeting to Dr. Hopkinson and yourself for bringing the matter forward will, we hope, receive considerable confirmation, and will benefit largely the whole workers in electricity by the amplified use which these alternate-current machines will receive from that confirmation.

Professor G. Forbes asked Mr. Shoolbred whether he considered the shock he had received from an alternate-current machine as a confirmation of the suggestion of Professor Ayrton.

Mr. J. N. Shoolbred: I gathered that the statement, or rather query, put forward by Professor Ayrton had a direct connection with the circumstance that had previously occurred to myself, and which would very likely occur to anybody else, especially to some unfortunate housemaid who might approach the machine in any house where it might happen to be placed.

Professor G. Forbes: That answers my question: but surely you got the shock simply because you were in contact with two points of the wire, because of the line being leaky?

Mr. J. N. Shoolbred: I was in communication with the earth at the time.

Professor G. Forbes: Yes, because it was a leaky line.

Mr. J. N. Shoolbred: No doubt, it was very leaky.

Mr. R. E. Crompton: Mr. President,—I am myself entirely a learner in this very interesting subject. I want to ask Dr. Hopkinson a question. I think Dr. Hopkinson said that there was no doubt that if we had two alternating machines feeding a system of mains, they might, although placed a considerable distance apart, be worked so as to run synchronised with one another, and would get into phase so that we should have equal potential throughout the system. I want to ask him whether he really considers that this will practically do away with the trouble of coupling alternating machines on to a network of mains, such as has been proposed for the distribution of the electric current for lighting purposes. Because we have been told (I have not been investigating the subject at all lately, so I am purely in the position of a learner) that not only were troubles to be expected from the difficulty of getting the machines to synchronise, which difficulty is now apparently overcome, but also that difficulties would arise from retardation, owing to the self-induction of the mains themselves; that every branch would have a different coefficient of self-induction; and that, as a consequence, we should get waves and fluctuations in the current itself which would cause considerable fluctuations in the light. The fluctuations would be irregular in

extent and occur at irregular intervals of time. Such fluctuations have been observed in large installations worked by the alternating current, and these, I believe, have been put down to this self-induction of the mains. I should like to hear from Dr. Hopkinson his latest views on the subject, as to whether this effect really does exist, and whether there is hope of its being completely done away with.

Mr. W. M. MORDEY: I would like to ask a question, Sir. In electric lighting I suppose the main object in a given arc lamp, or in a given incandescent lamp, on a given circuit, is to get a certain amount of heat as economically as possible. I do not think that anything very clear has been published as to the relative efficiencies of alternate-current and continuous-current dynamos. I would therefore ask Dr. Hopkinson or Professor Adams if any definite information can be given to show, if we take the best obtainable alternate-current dynamo, and also the best obtainable continuous-current dynamo, which would give us, with a given circuit, the required heat in the lamp most economically.

Professor W. G. ADAMS: Perhaps, before Dr. Hopkinson replies to the questions which have been put, I may just say a word or two with regard to one or two points raised by Mr. Shoolbred. I may say that the communication which I made to the Society was really an account of that small section of the work that I have been doing at the South Foreland, which bore especially on the working of alternate-current machines together on the same circuit, my object being to bring forward illustrations of Dr. Hopkinson's theory.

We have heard a great deal with regard to the dangers of shocks from alternate-current machines, and certainly these machines at the South Foreland are not small ones; the electromotive force is not very high, about 80 volts each on open circuit, but at the same time the current is a very strong one, being, from a single machine, from 160 to 175 ampères through a single arc. The following may be of interest in regard to the shocks from such machines:—On one occasion two De Meritens machines were clutched together, and were worked in tension on the same simple circuit, to supply two arcs which were in series; and I was

making experiments on these two machines, determining the electro-motive force and the current in circuit. I had a Siemens electro-dynamometer to measure the current, and a Cardew's voltmeter to measure the electro-motive force. During my absence from the engine-room the engineer at the South Foreland wanted to make some alteration in the arrangement of the circuit, and wished to put the electro-dynamometer out of the circuit. To do this he took hold of one of the uncovered leading wires to the electro-dynamometer, having loosened the screw previously, intending to draw it out and break the circuit; but the wire did not come very readily, and consequently, without thinking any more about it, he put his hand on the binding-screw, to prevent the instrument from being pulled over, and then broke the contact between the lead and the binding-screw of the electro-dynamometer. The consequence was that he himself formed the only means of communication for the current between the two parts of the circuit. He drew out the leading wire, having the binding-screw in the other hand at the time. The electro-motive force between the two terminals was about 160 volts with the circuit open, and was about 75 volts with the circuit closed. The current was about 130 to 140 ampères, going through the main circuit before the break. As soon as the engineer had broken contact he fell down, pulling the instrument with him, but did not lose consciousness. Immediately after the shock he attempted to get up and to put the apparatus in its place, but he was quite white in the face, and was advised by those who were present to sit down and rest for a few minutes. I was not present at the time, but saw him within ten minutes; he was then quite himself again, and had all his wits about him, thoroughly well, and apparently not affected in the least. This incident adds a little to our knowledge with regard to the shocks from alternate-current machines, and seems to show that those shocks are not so fearful as might previously have been expected. One would say, for instance, that the effect of breaking the contact of a continuous current of anything like the same intensity would certainly be something excessively severe in comparison with what actually occurred in this case.

Mr. R. E. CROMPTON: Was it 75 or 150 volts?

Professor W. G. ADAMS: The two machines together gave 75 volts at the terminal with the circuit closed through two arcs, but the electro-motive force from the two machines was 160 volts on open circuit.

Mr. I. S. BEEMAN: May I ask if the man was making metallic connection with both hands, or had he an insulated conductor in one hand?

Professor W. G. ADAMS: He had simply an uncovered copper wire rope in one hand, and with the other hand he took hold of the binding-screw of the electro-dynamometer from which he withdrew this copper wire.

Dr. J. H. HOPKINSON: In the remarks that I have to make in reply, perhaps it would be most convenient if I followed the speakers in the order in which their remarks were made. Mr. Alexander Siemens gave us some exceedingly interesting results of experiments which have been made at Woolwich. I need hardly say that it was very unnecessary for him to apologise to me in any way for bringing those results forward; indeed it was a considerable gratification to me to find that the theoretical conclusions had excited so much interest. Professor Ayrton and Professor Silvanus Thompson both alluded to the question of the accuracy of Joubert's formula, which really underlies the work which I have given in the paper. As I mentioned in the paper, Joubert's formula is not strictly accurate; there are one or two slight corrections which it would require for some classes of machines. For example, the self-induction of the secondary circuit in any alternate-current machine is not an absolute constant, but depends upon the position of the coil in relation to the magnets to a certain extent, though I believe it is very nearly constant, and for practical purposes may be so regarded. That would produce a slight correction. Professor Ayrton, however, seemed to be led to the conclusion that the effect of the secondary current itself in modifying the field in which it was moving was not taken into account, but in point of fact it is taken into account in the coefficient of self-induction. There are really two ways of looking at self-induction, and they both come to the same thing

and give precisely the same results. One way is that in which I think Faraday looked at it. Faraday considered that the circuit itself produced a magnetic field, the variations of which had an effect as electro-motive force upon that circuit. Another way of looking at it is that which Clerk Maxwell introduced, of treating the current in a circuit as possessing the property of momentum, much as water in a pipe possesses the property of momentum. I do not think it very much matters which way we adopt; they both come to the same thing, and it is largely a question of convenience which should be used in any particular case. When it is a case in which there are iron magnets in the neighbourhood of the circuit, I am inclined to think that the old method of Faraday is probably the more convenient, but I doubt whether really the wisest plan is not to be ready to adopt either mode of expressing the facts, as may be most convenient as the cases arise.

Professor Silvanus Thompson alluded to the possibility of using condensers for the purpose of obtaining high potentials, and whether it was really what I may call practical politics to connect a condenser with an alternate-current machine. I do not suppose it is at present. I think that if you want to have a high potential, such as is obtained on coupling a condenser to an alternate-current machine, probably the best way is to make machines for the purpose. I doubt whether, practically, any beneficial result could be obtained from the use of a condenser as I have suggested; nevertheless, I think the problem is an instructive one.

Mr. Harrison remarked that I only treated in these formulæ with the simplest case in which the electro-motive force was represented by the simple sine or cosine of the time. I alluded to that at the very beginning of the paper, and said that it would be convenient just to take a simple term as a type, and that it was no use, when one term will serve the purpose, to write down more. No doubt the other terms may have a modifying effect on the results, but it has been shown by Joubert long ago, and by others since, I think, that the effect is small. I know my own experiments show that the principal part of the electro-motive force may be represented by the first term of the series, and I do not myself see that there is any great use in going into complex analysis, and making formulæ look more formidable than they really need be,

for the sake of giving a completeness which is not required for the questions which are discussed.

The discussion this evening was opened by Mr. Blakesley, and I certainly think that his method of treating the subject is one that is very well worthy of our attention. To many, a geometrical treatment is easier than an analytical one; for my own part I generally find that I can get along faster with the analysis, and therefore, as a rule, I naturally bring it into use; but, for all that, I think to have the thing put from two points of view is a very great advantage.

Mr. Kapp asked how I could account for what appeared to be an enormous disappearance of energy without any corresponding heat. The fact is, there is no such disappearance of energy in the case which he describes. The result obtained by multiplying the mean electro-motive force by the mean current would be very far from the true energy developed between the terminals. The fact is that the maximum current is not passing in that case at the same time that the electro-motive force is at the maximum, and consequently the product of the two is by no means the large quantity which Mr. Kapp would suppose. The effect of self-induction comes in, but not in the way of wasted energy. Mr. Shoolbred alluded to the question of shocks. I am very glad to see Mr. Shoolbred here this evening. I am not at all sure whether there is any conclusive evidence as yet that alternate currents do give greater shocks than continuous currents. I think it is quite possible that it may turn out to be the fact that, taking the maximum electro-motive force that occurs in an alternate-current machine, its shock might be somewhat comparable with the shock given by a corresponding force with a continuous-current machine. I do not know that experiments yet prove what is the truth of that matter. It is true that several deaths have occurred with alternate currents, but those alternate currents have been of high potential. The subject of the effect of shocks is one of extreme difficulty, because, as has been pointed out by Dr. Stone, it depends on the contact obtained. I believe it was observed by Cromwell Varley that from two or three battery cells very severe shocks could be obtained if care was taken to make good contact, such as would be caused by immersing the arms or hands in basins

of water and then connecting up the battery. I never tried the experiment myself, but I know that some observations on the point were made long ago. Mr. Crompton referred to the evidence we gave with regard to the Electric Lighting Provisional Orders some time ago. I do not remember at this moment whether I said that two alternate-current machines could not be successfully connected to the same circuit, but, if I did, I should not have said anything but what I and every one believed was the fact, because it was a matter of opinion which I suppose we all shared, and in which we were no doubt all wrong. But now the matter is a little clearer, and if we had happened to be aware of what Mr. Wilde had done years ago, we should have been wiser then than we were. Mr. Crompton asked what my views were as regards connecting a network of conductors from a number of alternate-current machines in different places. I have not tried the experiment, but, as far as I can see, the thing is perfectly practical. I do not think it would be very materially interfered with by the self-induction of the mains, which cannot be a very large quantity, and must be of moderate dimensions when two conductors are used, a conductor going out and a conductor returning, because then the inductive effect of the one is partially neutralised by the inductive effect of the other. Mr. Mordey asked a question with regard to the efficiency of alternate-current machines. The efficiency of the De Meritens machine certainly is very high, as appears by the results I have given in the paper. It will be seen that, for instance, in the case when the power measured on the belt was 6·5 horse-power, the electrical power developed was 5·7 horse-power. That was determined by means of measuring the current passing through a resistance with no sensible self-induction, and therefore was free from objection. The figures are those which were determined at the time, and were of course based on the assumption that the B.A. unit was a true ohm, and consequently the 88 per cent. of efficiency would be rather too high; but still I may say that the efficiency of that machine, tested as it was four years ago, was higher than the efficiency of any other machine that was in existence at the time, unless it be some alternate-current machine which I have not tried. Of course it was only a magneto machine,

and consequently it rather goes to show that there is no reason to suppose that alternate-current machines are inferior, necessarily, than continuous-current machines. Now, of course, higher efficiency than 88 per cent. is obtainable with continuous currents.

I may perhaps mention an experiment which I tried upon two De Meritens machines which we had running for the lighthouse at Tino, which showed the convenience with which they could be run together. In each machine the circuit was divisible into two parts, so that they could be worked at half or full power. On one occasion the two machines were running in parallel circuit together, giving a powerful arc. Owing to a little maladjustment at the collecting brushes, the contact was not good; there was a little spark formed. We had no difficulty in taking off the brush while running, without disturbing the harmony in the machines; the current fell to three-fourths of the value it had when the two machines were working at full power. It was easy to change from one machine to the other without extinguishing the light: we could reduce one machine to half-power, giving a light of half-power; we could then throw in the other machine at half-power, consequently the two half-powers would make the whole power of one machine; then we could break the circuit of the first machine, and finally throw in the second, and so change from one to the other without extinguishing the light for a moment. That is a matter of some importance in using machines of this kind for lighthouse work. Of course we all know that the same thing could be done with even greater facility with two continuous-current machines.

A hearty vote of thanks was unanimously passed to Dr. J. H. Hopkinson and Professor Adams for their respective communications.

A ballot took place at which the following were elected:—

Associates:

Capt. Martin Julius Dunlop, R.N.	Clement Joachim.
Henry Newman Lawrence.	James Stewart.

Robert Mullineux Walmsley, B.Sc., F.C.S.

The meeting then adjourned until Thursday, 11th December, 1884, for the annual meeting.

The Thirteenth Annual General Meeting of the Society was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, 11th Dec., 1884—Professor W. G. ADAMS, F.R.S., President, in the Chair.

The PRESIDENT announced that the ballot-box for the election of Members of Council and Officers would remain open until 8.30 p.m.

The minutes of the previous meeting were read and approved.

The names of new candidates were announced and suspended.

Mr. Le Neve Foster and Mr. J. Bailey were appointed scrutineers of the ballot.

Donations to the Library were announced as having been received from the following:—A. L. Ternant, Member; J. Angelo Fahie, Member; Messrs. Trübner & Co.; Don Arthur Indio do Brazil; and Herr E. Obach, to all of whom the thanks of the meeting were heartily accorded.

The SECRETARY then read the following Report of the Council:

REPORT OF THE COUNCIL PRESENTED TO THE ANNUAL GENERAL MEETING OF THE SOCIETY.

11TH DECEMBER, 1884.

Your Council are enabled to report that the Society continues to increase in numbers.

The additions made during the year are as follows:—1 Hon. Member, 12 Foreign Members, 7 Members, 62 Associates, and 14 Students—total, 96.

Besides these, 4 candidates have been approved for ballot this evening, and 20 for ballot at our first meeting next month.

By deaths and resignations our losses are as follows:—1 Hon. Member, 9 Foreign Members, 8 Members, and 17 Associates—total, 35.

Among the deaths, we have to lament the loss of Mr. Frank Ives Scudamore, C.B., Past-President and Honorary Member, who, as Secretary to the Post Office, took so important a part in the transfer of the telegraphs to that department; also of Mr. Robert

Sabine, one of the original members of the Society, and its first Treasurer; and of the Count du Moncel, one of the most distinguished of our Foreign Members.

As announced by the President to the General Meeting of November 13th, the Council have unanimously elected Mr. Edward Davy, M.R.C.S., an Honorary Member of the Society, in recognition of his early inventions and investigations in connection with the electric telegraph.

Although Mr. Davy's labours are referred to in several works on electricity, yet, owing to his leaving England for Australia, and his consequent abandonment of the subject, the full extent and value of his experimental researches in connection with the application of electricity to telegraphic purposes were not fully known. Quite recently, however, these have been brought prominently forward through the publication, by our member Mr. J. J. Fahie, of Mr. Davy's early correspondence and writings, the originals of which have been kindly presented to the Society's Library by his nephew, Dr. Henry Davy, and form a valuable and interesting contribution to the history of electric telegraphy.

The Society continues to enjoy the privilege of holding its general meetings in the theatre of the Institution of Civil Engineers.

Of the papers read during the year, a list of which is subjoined, many have proved of much interest. In respect of those eligible* for competition for premiums, the Council have made the following awards, viz.:—

The Society's Premium, value £10, to Professor George Forbes, F.R.S.E., Member, for his paper "On the Relation which should subsist between the Strength of an Electric Current and the Diameter of Conductors, to prevent Overheating."

The Fahie Premium, value £5, to W. H. Stone, M.A., M.B., F.R.C.P., Member, for his paper on "The Physiological Bearing of Electricity on Health."

The Paris Electrical Exhibition Premium, value £5, to Henry C. Mance, Member, for his paper "On a Method of

* Papers by Members of the Council are ineligible.

Eliminating the Effects of Polarisation and Earth Currents from Fault Tests."

The Council desire to make honourable mention of the paper "On some Prejudicial Actions in Dynamo Machines," by Mr. W. Mordey, Associate.

LIST OF PAPERS READ DURING THE SESSION 1884.

Date.	Title.	Author
Jan. 31.	—On a System of Electric Fire-Alarms	Edward B. Bright, M.Inst.C.E., Member.
Feb. 14.	—On some New Instruments for Indicating Current and Electro-motive Force	R. E. Crompton and G. Kapp, Members.
„ 28.	—On some Prejudicial Actions in Dynamo Machines	W. M. Mordey, Associate.
„ 28.	—Breguét's Telephone ... ... Compensated Resistances... ...	Prof. George Forbes, F.R.S.E. Member.
Mar. 13.	—Notes on a Train-lighting Experiment	W. H. Massey, Member.
„ 27.	—On the Relation which ought to subsist between the Strength of an Electric Current and the Diameter of Conductors, to prevent Overheating	Prof. G. Forbes, F.R.S.E., Memb.
April 24.	—On the Relation which should subsist between a Current of Electricity and the Conductor employed to convey it	Thomas H. Blakesley, Assoc. Memb. Inst .C.E.
May 8.	—On a Method of Eliminating the Effects of Polarisation and Earth Currents from Fault Tests ...	Henry C. Mance, C.I.E., Member.
	With Supplementary Remarks and Illustrative Experiments ...	Latimer Clark, Past-President.
„ 28.	—On the Electrical Congresses at Paris	W. H. Preece, F.R.S., Past-President.
July 4.	—Read at the Society's Conference at the International Health Exhibition, South Kensington:—	
	Artificial Lighting in Relation to Health	R. E. B. Crompton, Member.
	The Physiological Bearing of Electricity on Health...	W. H. Stone, M.A., M.B., F.R.C.P., Member.

Date.	Title.	Author.
Nov. 13.—	On the Theory of Alternating Currents, particularly in reference to two Alternate-current Machines connected to the same Circuit	Dr. J. Hopkinson, F.R.S., Member.
„ 13.—	The Alternate-current Machine as a Motor...	Prof. W. Grylls Adams, F.R.S., President.
Dec. 11.—	Electricity in America in 1884 ...	W. H. Preece, F.R.S., Past-President.

At the invitation of the Executive Committee of the International Health Exhibition, a Conference of the Society was held at the Exhibition on July 4th, when, as noted above, two interesting and valuable papers were read by Mr. R. E. Crompton and Dr. W. H. Stone, Members.

The very agreeable Reunion given by Mr. Willoughby Smith, as President of the Society, last year, at the South Kensington Museum, was followed this year by a numerously attended Conversazione given by Professor Adams in the Museum, Physical Laboratory, and Art Galleries of King's College, when to the valuable collection of scientific apparatus belonging to the College, a large number of very interesting exhibits were added, for the occasion, by members and others.

As will be seen by the Librarian's Report, appended hereto, the accessions to the Library have, through the liberality of our members and others, and the assiduity of Mr. Frost, been very numerous and valuable, independently of works purchased by the Society.

The gentlemen who represent the Society as Local Honorary Secretaries abroad, continue to do good service in acting as mediums of communication between the members in their respective secretariats and the chief office in London. The Council regret to say that Mr. James Dakers, who has for so many years represented the Society in Canada, has, through ill-health, been compelled to retire. Mr. Angus Grant, of the Great North-Western Telegraph Company of Canada, has accepted office in his place.

The financial position of the Society continues to be satisfactory.

LIBRARIAN'S REPORT.

F. H. WEBB, Esq., *December* 11*th*, 1884.

Secretary,

Society of Telegraph-Engineers and Electricians.

DEAR SIR,

I beg to hand you, for the information of the Council, my Fifth Annual Report upon the Library of the Society.

The number of members who have used the Library during the twelve months ending December 1st, is in excess of any previous year, while there has been a slight falling-off in the number of other visitors. That the attendance is not greater, may be possibly due to the fact that it is not generally known that the Library is open till 8 o'clock p.m. on four evenings in each week. The visitors' book shows that the attendance during the past four years has been as follows:—

	1881.	1882.	1883.	1884.
Members	180	313	403	429
Non-Members...	158	248	240	162
Total	338	561	643	591

The collection of electrical specifications of patents has been carefully kept up during the year, and those recently published are week by week placed on the Library table for reference. The total number of applications for patents since 1st January to the 1st December amounts to 15,819. Of these no less than 1,095, or nearly 7 per cent., had reference to electricity and its applications. The following figures will show the number of electrical patents which have been applied for during the past six years:—

1879.	1880.	1881.	1882.	1883.	1884.
300	260	450	845	620	1,095

The specifications in the Library have been compared with those referred to in the latest abridgments which have been issued from the Patent Office, and it has been found that many important specifications were omitted from the two volumes of Abridgments which were published in 1859 and 1870. As the Society's collection was compiled from these volumes, the omissions have been noted and will be applied for, and afterwards added to the Library. The latest abridgments of electrical specifications which have been published only extend to the year 1876, far too late to be of much practical value. Valuable time would be saved in reference, if these abridgments were more frequently published; and it is to be hoped that the time is not far distant when a good practical classified index may be compiled and issued periodically. The thanks of the Society are due to H.M. Commissioners of Patents, and to Mr. R. Morris, the Superintendent of the Store Department, for the assistance which we receive in obtaining the specifications as soon as published.

The additions to the Library during the year have been very numerous, and amount to over 600 books and pamphlets. Of these, the presentations form by far the largest proportion, many of them being of the utmost importance and value. The list of accessions have been printed from time to time in the Journal of the Society, and, with those appended hereto, form a fairly good record of the most important works issued during the year. The number of volumes and pamphlets added to the Library by presentation and purchase during the past four years is given below:—

	1881.	1882.	1883.	1884.
By Presentation	156	151	204	530
„ Purchase	52	94	284	76
Total	208	245	488	606

Exchanges have been effected with the American Academy of Science and Arts, the Royal Dublin Society, the Franklin Institute, the Institution of Civil Engineers, the Ordnance Depart-

ment of the United States, and the Institute of Patent Agents, and with several other scientific periodical publications.

The Society's collection of current periodicals is a very important one; and appended hereto will be found a list of those which are regularly received, and which are always available for reference.

Amongst the donors to the Library during the year, special reference should be made to the valuable presentation of 320 volumes by Lady Siemens, from the library of the late Sir William Siemens, D.C.L., F.R.S., Past-President (these books I selected from lists submitted to me by the desire of Lady Siemens); a collection of works from the library of the late General Sir E. Sabine, K.C.B., F.R.S., presented by G. J. Symons, Esq., F.R.S., being the second donation of books from this gentleman; a complete set of the proceedings in the case of The American Bell Telephone Company *v.* The People's Telephone Company, eight volumes, which were presented, I believe, by the American Bell Telephone Co.; an important collection, very nearly complete, of the Arbitration Papers relating to the Railway Companies and the Post Office, presented by R. Price Williams, Esq., M.Inst.C.E.; and a small parcel of interesting pamphlets presented by Hyde Clarke, Esq.

The classified catalogue of electrical literature which I am preparing, and to which I referred in my last Report, is rapidly approaching completion. The cards are all mounted and partly arranged, and I hope in the course of the ensuing year to report its completion and submit it to the Council. The work of arranging the titles under suitable headings is one of some difficulty; but those relating to the most important of the practical branches of our subjects, such as electric lighting, telegraphy, medical electricity, lightning, meteorology, telephony, etc., are so nearly completed that in the course of a few weeks they will be available to the members for reference. This arrangement will materially assist those who desire to refer to works on special subjects, but who are not acquainted with the names of the authors.

The amount voted for Library purposes, and which includes

the purchase and binding of books and periodicals, is not sufficiently large to admit of many purchases of works which are required to complete the Library, or to purchase those works which Ronalds noted but did not obtain; but the Catalogue of Accessions shows that something has been done during the year to supply the vacancies.

The rapidity with which the Library is growing, and the large number of works added from time to time, have necessitated an extension of shelving space during the year. This space has been utilised to the utmost extent, and it will be necessary shortly to make a still further extension.

As it is desirable that the Library should be made as practically useful as possible, I shall be glad to receive the co-operation of the members, both in the presentation of books and in any suggestions with which they may favour me.

I am,

Dear Sir,

Yours faithfully,

A. J. FROST,

Librarian.

4, The Sanctuary,
Westminster, S.W.

APPENDIX TO LIBRARIAN'S REPORT.

LIST OF PERIODICALS RECEIVED BY THE SOCIETY.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Cambridge Philosophical Society, Proceedings.
Electrical Engineer.
Electrician.
Engineer.
Engineering.
English Mechanic and World of Science.
Greenwich Magnetical and Meteorological Observations.
Incorporated Law Society Calendar.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
Journal of Science.
Library Chronicle.

Military Telegraph Bulletin.
Nature.
Patents' Journal, Commissioners of.
Philosophical Magazine.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings
Royal Engineers' Institute, Proceedings.
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal United Service Institution, Proceedings.
Society of Arts' Journal.
Society of Engineers, Proceedings.
Telegraphic Journal and Electrical Review.
Telegraphist.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
Electrical Review.
Electrician and Electrical Engineer.
Franklin Institute, Journal of.
John Hopkins University Calendar.
Journal of the Telegraph.
Library of Cornell University.
Ordnance Department of the United States.
Science.
Scientific American.
Smithsonian Institution Reports.
United States Patent Office, Official Gazette of.

FRENCH.

Annales de l'Electricité.
Annales Télégraphiques.
Bulletin de la Compagnie Internationale des Téléphones.
Cosmos les Mondes.
Journal de Physique.
Journal du Gaz et de l'Electricité.
Journal Télégraphique.
La Lumière Electrique.
Le Franklin.
L'Electricité.
L'Electricien.
Société Belge d'Electriciens, Bulletin de la.
Société Française de Physique, Séances de la.
Société des Ingenieurs Civils, Mémoires.
Société Internationale des Electriciens, Bulletin de la.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Centralblatt für Elektrotechnik.
Der Elektro Techniker.
Electrotechnische Zeitschrift.
Electro-technischer Anzeiger.
Repertorium für Experimental-Physik für Physikalische-Tecknik.
Verhandlungen des Vereins zur Beforderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentenkunde.

ITALIAN.

Government Telegraph Department, Annual Report.
Il Telegrafista.

RUSSIAN.

Government Telegraph Department, Annual Report.

SPANISH.

La Electricidád.

Mr. H. C. FORDE remarked that the brief mention made in the report to the Institution of Civil Engineers was, in his opinion, insufficient to properly express the feelings of the Society.

The PRESIDENT reminded Mr. Forde that it was the practice to pass a special vote of thanks to the Institution of Civil Engineers for their liberality, and such course was about to be followed, and moved that the report of the Council be received, adopted, and printed as part of the Society's Journal. The report was short, but its shortest paragraph contained a statement of the greatest interest, and that was in the brief sentence with which it concluded, that the "financial position of the Society continued to be satisfactory."

Professor FORBES having seconded the motion, the report was unanimously adopted.

The PRESIDENT: I have very great pleasure indeed in moving that the cordial thanks of the Society be presented to the President, Council, and Members of the Institution of Civil Engineers for their kindness and liberality in continuing to permit the Society to hold their general meetings in the theatre of their Institution. It is impossible for us to estimate this

privilege too highly. It is an exceedingly valuable privilege indeed that we should be allowed the use of such an excellent room as this for our meetings. In fact, the success of the Society may be said greatly to depend upon the kindness and liberality of the Institution of Civil Engineers; and therefore we cannot express too heartily what we must all feel very deeply, that our best thanks are due to the President, Council, and Members of the Institution of Civil Engineers.

Mr. SPAGNOLETTI seconded the proposition, which was enthusiastically carried.

The PRESIDENT proposed, and Mr. ANSELL seconded, a hearty vote of thanks to the Local Honorary Secretaries and Treasurers abroad for their kind attention to the interests of the Society.—Carried unanimously.

Mr. LE NEVE FOSTER proposed a vote of thanks to Mr. Edward Graves for his continued attention to the financial interests of the Society, as Honorary Treasurer.

Mr. E. B. BRIGHT seconded the proposition, and referred to the great work Mr. Graves had done for the Society, and the great care he had taken with its finances. When they knew that a good hand was at the helm, ready to prevent too much expenditure or insufficient collection, they were very fortunate, and Mr. Graves had always shown himself to be the right man in the right place.—Carried unanimously.

Mr. EDWARD GRAVES, in acknowledging the vote of thanks, called attention to the fact that for some time the Society had neglected a duty which was incumbent upon them, and which neglect, perhaps, an immediate step would be taken to repair. The Auditors of the Society had for many years devoted their time and attention to its accounts, to ascertain how they really stood. Since its registration under the Limited Liability Act their duties had become more onerous, and it was necessary to go with greater detail than before; and, as no recognition had hitherto been made of the Auditors' services, Mr. Graves begged to move that the best thanks of the Society be presented to Mr. J. Wagstaff Blundell and Mr. Fred. C. Danvers for their services as Auditors.

Seconded by Professor HUGHES, and carried unanimously.

Mr. E. B. BRIGHT: It is always a very pleasant duty to bring before such a Society as this, where we all have one object, and where we all know through what a deal of work the Society has had to go in order to become legally constituted, a vote of thanks to those who have put us into our present position. We have been steered through many legal troubles that naturally arose in the first formation of the Society, and during its continuance, and have also been safely brought into our present legal position, by our good friend the Honorary Solicitor to the Society, Mr. G. L. Bristow, and therefore it is a great pleasure to me to be allowed to propose a vote of thanks to him for all the trouble that he has taken for years in seeing us through what might have proved legal shoals. I am quite sure that we all thank him most heartily for the legal advice he has so long given to the Society.

Seconded by Mr. H. C. FORDE.

The PRESIDENT: I am quite sure that in past years Mr. Bristow has had a great deal of work to do for us, and we are exceedingly obliged to him; and I will ask you to pass a cordial vote of thanks to him, as our Honorary Solicitor, for the able way in which he has conducted us through our difficulties, and safely landed us in the position of a registered society.

The vote was heartily accorded.

Mr. G. L. BRISTOW: I am sure, Mr. President, that I am very much obliged to you for the very flattering manner in which the Society has been kind enough to pass a vote of thanks to me; but really during the past year my duties have been about "nil."

The ballot-box was withdrawn.

The following lecture was delivered on

A VISIT TO CANADA AND THE UNITED STATES IN THE YEAR 1884.

By MR. W. H. PREECE, F.R.S.

I do not know what the sensations of a man can be who is about to undergo the painful operation of execution; but I am inclined to think that his sensations must be somewhat similar to

those of a lecturer, brimful of notes, who has to wait until the clock strikes before he is allowed to address his audience.

The President has been kind enough to refer to the paper I propose to give you, as "Electricity in America in the year 1884;" but I would rather, after having thought more about it, that it be called "A Visit to Canada and the United States in the year 1884."

It will be in the recollection of a good many who are present that in the year 1877 I visited America, in conjunction with Mr. H. C. Fischer, the Controller of our Central Telegraph Station, to officially inspect and report upon the telegraph arrangements of that country; and on the 9th February, 1878, I had the pleasure of communicating to the members of this Society my experiences of that visit.

During the present year my visit was not an official one: I went for a holiday, and specially to accompany the members of the British Association, who, for the first time in the history of that association, held a meeting outside the limits of the United Kingdom.

We sailed from Liverpool in a splendid steamship called the "Parisian." There were nearly 200 B.A. members on board; and notwithstanding the fact that rude Boreas tried all he could to prevent us from reaching the other side of the Atlantic; notwithstanding the fact that the Atlantic expressed its anger in most unmistakable terms at our audacity in turning from our native shore; notwithstanding the fact that Greenland's icy mountains blew chilly blasts upon us and made us call out all the warm things we possessed—I say notwithstanding all this, we reached the Gulf of St. Lawrence in safety, and I do not think that a merrier or a happier crew ever crossed the Atlantic.

There is one very interesting fact that is not generally known, and I certainly was unaware of it before I started, in connection with this particular route across the Atlantic, and that is, that by it the ship passes within only 200 miles of Greenland. The great circle that directs the shortest route from the north of Ireland to the Straits of Belle Isle passes within the cold region, and hence, while you were all sweltering in heat in London, we were

compelled to bring out our ulsters and all our warm garments, to enable us to cross with any degree of comfort. The advantage of this particular route is supposed to be the fact that only five days are spent upon the ocean, and the remainder of the voyage is occupied in the calms and comforts of the Gulf and River St. Lawrence. But I am inclined to think that the roughness of the ocean and the coldness of the weather at all seasons are quite sufficient to prevent anybody from repeating our experience.

We arrived at Montreal in time to attend the opening meeting of the British Association; and at Montreal we were received with great hospitality, great attention, and great kindness from all our brethren in Canada, and we held there certainly a very successful and very pleasant gathering. There were 1,773 members of the British Association altogether present, and of that number there were 600 who had crossed the Atlantic; the remainder being made up of Canadians, and by at least 200 Americans, including all the most distinguished professors who adorn the rolls of science in the United States. As is invariably the rule in these British Association meetings, we had not only papers to enlighten us, but entertainments to cheer us; and excursions were arranged in every direction, to enable us to become acquainted with the beauties and peculiarities of the American Continent. Some members went to Quebec, some to Ottawa, others to the Lakes, others to Toronto, many went to Niagara; and altogether the arrangements made for our comfort and pleasure were such, that I have not heard one single soul who attended this meeting at Montreal express the slightest regret that he crossed the Atlantic.

The meeting at Montreal certainly cannot be called an electricians' meeting. The gathering of the British Association has often been distinguished by the first appearance of some new instrument, or the divulgence of some new scientific secret; but there was nothing of any special interest brought forward on this occasion. The only real novelty or striking fact that I can recall as having taken place was a remarkable discussion that was originated by Professor Oliver Lodge, upon the "Seat of the Electro-motive Force in a Voltaic Cell."

This was an experiment on the part of the British Association. Discussions, as a rule, have not been the case at our meetings. Papers have been read and papers have been discussed; but on this occasion three or four subjects were named as fit for discussion, and distinguished professors were selected to open the discussion.

On this particular subject, Professor Oliver Lodge opened the discussion, and he did so in an original, an efficient, and a chirpy kind of manner, that took by storm not only the professors who knew him, but those who did not know him; and I am bound to say that I do not think we could possibly better spend an evening during the coming session, or more profitably, than by asking Professor Oliver Lodge to bring the subject before this Society, so as to allow us on this side of the water to discuss the same subject.

Of course the prominent figure at our meetings was Lord Rayleigh; and I do not think any person could possibly have been present at those meetings of the British Association without feeling an intense personal admiration for this man, and an affection for the way in which he maintained the position of an English gentleman, and the credit of an English scientific body, to the astonishment and delight of every one present. Then, again, we had our Past-President Sir William Thomson, who was not quite so ubiquitous as usual: he did not dance from section to section as he usually does, but remained as president of his own section, A. I think he only left his section for one day, and that was to attend the electrical day in Section G; but in his own section he brought down those words of wisdom that one always hears from him, and which make one always regret that there is not always present about him a shorthand writer to take down thoughts and ideas that never occur again, and are only heard by those who have the benefit of being present.

The subjects brought forward were not of intense interest. We had a paper by Dr. Traill, describing the Portrush Railway; and there were various other papers; and I can pass over some of the other subjects, because I shall have to deal with them under another head. But while we were in Montreal, a deputation of American professors and members of the American Association came over, and invited a good many of those who were present at

Montreal to visit the American Association at Philadelphia. I was one of those who went over to America simply and solely for a holiday, and I am bound to say that I set my face determinedly against going to Philadelphia. I travelled with two charming companions, and we all decided not to go to Philadelphia. But the compact was broken, and we capitulated, and went from the charming climate of Montreal into the most intense heat, and into the greatest discomfort that I think poor members of the Telegraph-Engineers' Society ever experienced. We entered a heat that was 100° by day and 98° by night; and I do not think there is anybody in this room, unless he has been brought up in the furnace-room of an Atlantic steamer, who can fully appreciate the heat of Philadelphia in these summer months. The discomforts of the climate were, however, amply compensated for by the hospitality and kindness of the inhabitants. We spent, in spite of the heat, a very pleasant time.

Before referring further to the meetings at Philadelphia, I may just mention the other journeys that I took. My holiday having been broken by the rupture of the union to which I have alluded, I had to devote it then to other purposes, and, in addition to Montreal and Philadelphia, I went to New York (to which I shall refer again), from New York to Buffalo, then to Lake Erie and Cleveland, and on to Chicago, where I spent a week or more. From Chicago I went to see the great artery of the West—the Mississippi: I stopped for a day or two at St. Louis. One remarkable fact came to my knowledge, and I dare say it is new to many present, and that is, that the Mississippi, unlike other rivers, runs up hill. It happens, rather curiously, that, owing to the earth being an oblate spheroid, the difference between the source of the Mississippi and the centre of the earth is less than that of its mouth and the centre of the earth, and you may see how this running up hill is accounted for.

From St. Louis I went to Indianapolis, thence to Pittsburg, where they have struck most extraordinary wells of natural gas. Borings are made in the earth from the crust to a depth of 600 or 700 feet, when large reservoirs of natural gas are "struck." The town is lighted by this gas, and it is also employed for

motive power. In Cleveland, also, this natural gas is found, and there is no doubt that it is going to economise the cost of production very much in that part of the country. From Pittsburg I went to Baltimore, where Sir William Thomson was occupied in delivering lectures to the students of the John Hopkins University. In all these American towns one very curious feature is that they all have great educational establishments, endowed and formed by private munificence. In Canada there is the McGill University, and in nearly every place one goes to there is a university, like the John Hopkins at Baltimore, where John Hopkins left 3,500,000 dollars to be devoted entirely to educational purposes; and that university is under the management of one of the most enlightened men in America, Professor Gillman, and he has as his lieutenants Professors Rowland, Mendenhall, and other well-known men, and each professor is in his own line particularly eminent. Sir William Thomson delivered there a really splendid course of lectures. From Baltimore I went through Philadelphia to Boston. I visited Long Branch, and I spent a long time in New York, so that from what I have said you will gather that I spent a good deal of my time in the States. Wherever I went I devoted all my leisure time to enquiry into the telegraphic, telephonic, and electric light arrangements in existence. I visited all the manufactories I could get to, and I did all I possibly could to enable me to return home and afford information, and perhaps amusement, to my fellow-members of this Society.

As an illustration of the intense heat we experienced, I may mention that it was at one time perfectly impossible to make the thermometer budge. The temperature of the blood is about 97 or 98 degrees, and if the temperature of the air be below the temperature of the blood, of course when the hand is applied to the thermometer the mercury rises. In one of our journeys up the Pennsylvania Road we tried to make the thermometer budge as usual, but could not, which proved that the temperature of the air inside the Pullman car in which we travelled was the same as that of the blood.

The American Association is of course based on the British Association. Its mode of administration is a little different. It is divided into sections, as is the British Association, but the sections are not called the same. For instance, in the British Association, Section A is devoted entirely to physics, but in the American Association, Section A is devoted to astronomy and Section B to physics. In the British Association, Section G is devoted to mechanics, but in America Section D is devoted to that subject. But with the exception of just a change in the names of some sections which are familiar as household words to members of the British Association, the proceedings of the American Association do not differ very much from ours. They have, however, one very sensible rule. The length of every paper is indicated upon the programme of the day's proceedings, and the continuation or the stopping of any discussion on that paper is in the hands of the section. For instance, if the President thinks that a man is speaking too long, he has only to say, "Does the meeting wish that this discussion shall be continued, or shall it be stopped?" A majority on the show of hands decides. Such a practice has a very wholesome effect in checking discussion, and I certainly think that some of our societies would do well to adopt a rule of the same character.

The meeting of the American Association, again, was not distinguished by any particular electrical paper, or any new electrical subject. The main subject that was brought before us was the peculiar effect, called "Hall's effect," that Professor Hall, now of Harvard College, and then assistant to Professor Rowland, discovered in the powerful field of a magnet when a current was passed through a conductor; and a description of that effect (which he at one time thought was an indication that electricity was something separate from matter) formed the subject of two debates that lasted for nearly the whole of two days. I am bound to say that in that prolonged discussion the members of this Society held their own. I see two very prominent members present who spoke on most of the electrical subjects dealt with—Professor G. Forbes, who knows what he says and says what he knows, and

Professor Silvanus Thompson, who held his own under very trying circumstances.

At the same time that this meeting of the American Association was being held at Philadelphia, where we were treated with marvellous hospitality,—excursions, soirées, dinners, parties, etc., etc.,—and as though it were not quite sufficient to bring over humble Britishers from this side of the Atlantic to suffer the intense heat at one meeting of the Association, they held at the same time an Electrical Conference. There was a conference of electricians appointed by the United States Government, that was chiefly distinguished on the part of the American Government by selecting those who were not electricians. But many attended the Electrical Conference who stand high as electricians, one especially, who, though perhaps from want of experience he did not shine very brilliantly as a chairman, certainly stands as one of the ablest electricians of the day—I mean Professor Rowland. The Conference was held under Professor Rowland's presidency, and nearly all the well-known professors of the United States attended. The Conference was established by the United States Government to take into consideration the results and conclusions arrived at by the Congress of 1884, held in Paris. The Paris Congress decided upon adopting certain units of resistance of electro-motive force, of current, and of quantity, and they determined the particular length of a column of mercury that should represent the ohm—a column of mercury 106 centimètres long and of one square millimètre in section. It was necessary that the United States should join this Conference, so a Commission was appointed to consider the whole matter. All these units were brought before them, as well as the other conclusions of the Paris Congress, such as the proper mode of recording earth currents and atmospheric electricity. The Paris units were adopted in face of the fact that the length determined upon at Paris was not the length that Professor Rowland himself had found as that which should represent the ohm. It differed by about ·2, as near as I can remember; but it was thought so necessary that uniformity and unanimity should exist all over the world in the adoption of a

proper unit, that all differences were laid aside, and the Americans agreed to comply with the resolutions of the Paris Congress.

There were two units that I had the temerity to bring forward, first, at the British Association, and secondly, before the Electrical Conference. It will be remembered that, at the meeting of the British Association at Southampton in 1882, the late Sir W. Siemens proposed that the unit of power should be the watt, and that the watt, which was derived from the C.G.S. system of absolute units, should in future, among electricians, be the unit of power. This was accepted by the British Association at Montreal, and it was also accepted by the American Electrical Conference at Philadelphia. But I also, at Montreal, suggested, that as the watt was the unit of power, so we ought to make some multiple of that unit the higher unit of power, comparable to that which is now represented by the well-known term "horse-power." Horse-power, unfortunately, does not form itself directly into the C.G.S. system. The term horse-power is a meaningless quantity; it is not a horse-power at all. It was established by the great Watt, who determined that the average power exerted by a horse was equal to about 22,000 foot pounds raised per minute; but this was thought by him to be too little, so he increased it by 50 per cent., and so arrived at what is the present horse-power, 33,000 foot-pounds raised per minute. Foot-pounds bear no relation to our C.G.S. system of units, and it is most desirable that we should have some unit of power, somewhere about the horse-power, to enable us to convert at once watts into horse-power. For that purpose I proposed that 1,000 watts, or the kilowatt, should replace what is now called the horse-power, and suggested it for the consideration of engineers. It has been received with a great deal of consideration by those who understand the subject, and a considerable amount of ridicule by those who do not. It is a rather remarkable thing that, as a rule, one will always find ridicule and ignorance running side by side; and it is an almost invariable fact that when a new proposition is brought forward it is laughed at. I am always very glad to see that, because it always succeeds in drawing attention to the matter. I remember a friend of mine, who had written a book, being in

great glee because it was severely criticised by the *Athenæum*, a fact which drew public attention to the book, and caused it to make a great stir. So when I proposed that the horse-power should be increased by 33 per cent., and made equivalent to 1,000 watts, I was not at all sorry to find that I had incurred the displeasure of the leader writers in nearly all our scientific papers, and I was quite sure that the attention of those who would not perhaps have thought of it would thereby be drawn to the matter. Some people object to the use of a name, this name "watt." When you have fresh ideas, you must have fresh words to express those ideas. The watt was a new unit, it must be called by some name, otherwise it could scarcely be conveyed to our minds. The foot, the gallon, the yard, were all new names once; and how do we know that they were not derived from some "John Foot," "William Gallon," or "Jack Yard," or some man whose name was connected with the measure when introduced? The poet says:

> "Some mute, inglorious Milton here may rest—
> Some Cromwell, guiltless of his country's blood:"

so in these names some forgotten physicist or mute engineer may be buried. At any rate, we cannot do without names. The ohm, the ampère, the volt, are merely words that express ideas that we all understand; and so does the watt, and so will the 1,000 watts when you come to think over the matter as much as some of us have done.

At this Conference several other subjects were brought up which attracted a good deal of attention. Professor Rowland brought forward a paper on the theory of dynamos, that certainly startled a good many of us; and it led to a discussion that is admirably reported in our scientific papers. I think that the discussion evolved by Professor Rowland's paper on the theory of dynamos deserves the study of every electrician: it brought very strongly into prominence one or two English gentlemen who were present. Professor Fitzgerald, of Dublin, spoke with a considerable amount of power, and showed a mastery of the subject that was pleasant not only to his friends, but must have been gratifying to the Americans who heard him. On this particular subject of dynamos it was truly wonderful how the doctors disagreed. Two

could not be found who held the same views on the theory and construction of the dynamo; and that shows that we still have a great deal to learn about the dynamo, and that the true principle of construction of it has yet to be brought out.

It is a very curious thing, and I thought about it at the time, that when you consider the dynamos in use, you see how very little has been done to perfect the direct working dynamo in England. Although the principle of the dynamo originated with Faraday, yet all the early machines, Paccinotti, Gramme, Hefner von Alteneck, Schuckert, Brush, Edison, and several others who have improved the direct action machine, have not been found in England. But when we deal with alternate-current machines, then we find the Wilde, Ferranti, and various others; so that the tendency in England has been very much to improve and work upon the alternate-current machines. In other countries it is exactly the reverse; in fact, in America I never saw one single alternate-current machine. When Professor Forbes wanted an alternate-current machine to illustrate a lecture that he gave, it was with the greatest difficulty that one could be found, and, in fact, it was put together specially for him.

The other subjects brought before this Conference were Earth Currents, Atmospheric Electricity, Accummulators or Secondary Batteries, and Telephones. There was an extremely able paper brought forward by Mr. T. D. Lockwood, the electrician of the American Bell Telephone Company, on Telephones, and the disturbances that influence their working. When that paper is published it will be well worth your careful examination.

Papers were also read on the Transmission of Energy, and there were papers on many other subjects.

So much for the Electrical Conference.

Now, the Americans at the present moment are suffering from a mania which we, happily, have passed through, that is, the mania of exhibitions.

While we were at Philadelphia there was an exceedingly interesting exhibition held. I do not intend to say much about that exhibition, for the simple reason that Professor G. Forbes has promised, during the forthcoming session, to give us a paper

describing what he saw there, and his studies at Philadelphia; and I am quite sure that it will be a paper worthy of him, and of you. But, apart from this exhibition at Philadelphia, I could not go anywhere without finding an exhibition. There was one at Chicago, another at St. Louis, another at Boston; everybody was talking about one at Louisville, where I did not go; and there were rumours of great preparations for the "largest exhibition the world has ever seen," according to their own account, at New Orleans. However, I satisfied myself with seeing the exhibition at Philadelphia, which consisted strictly of American goods, and was not of the international nature general to such exhibitions. But it was a fine exhibition, and one that no other single nation could bring together.

Telegraphs.—When I spoke to you in 1878, my remarks were almost entirely confined to telegraphs, for at that day the telephone was not, as a practical instrument, in existence. I brought from America on that occasion the first telephones that were brought to this country. Then the practical application of electricity was applied to telegraphs, and so telegraphs formed the subject of my theme. But while in 1877 I saw a great deal to learn, and picked up a great many wrinkles, and brought back from America a good many processes, I go back there now in 1884, seven years afterwards, and I do not find one single advance made—I come back with scarcely one single wrinkle; and, in fact, while we in England during those seven years have progressed with giant strides, in America, in telegraph matters, they have stood still. But their material progress has been marvellous. In 1877, the mileage of wire belonging to the Western Union Telegraph Company was 200,000 miles; in 1884, they have 433,726 miles of wire: so that during the seven years their mileage of wire has more than doubled. During the same period their number of messages has increased from 28,000,000 to over 40,000,000; their offices from 11,660 to 13,600; and the capital invested in their concern has increased from 40,000,000 dollars to 80,000,000 dollars—in fact, there is no more gigantic telegraph organisation in this world than this Western Union Telegraph Company. It is a remarkable undertaking, and I do not suppose there is an

administration better managed. But for some reason or other that I cannot account for, their scientific progress has not marched with their material progress, and invention has to a certain extent there ceased. There really was only one telegraphic novelty to be found in the States, and that was an instrument by Delany—a multiplex instrument by which six messages could be sent in one or other direction at the same time. It is an instrument that is dependent upon the principle introduced by Meyer, where time is divided into a certain number of sections, and where synchronous action is maintained between two instruments. This system has been worked out with great perfection in France by Baudot. We had a paper by Colonel Webber on the subject, before the Society, in which the process was fully described. Delany, in the States, has carried the process a little further, by making it applicable to ordinary Morse sending. On the Meyer and Baudot principle, the ordinary Morse sender has to wait for certain clicks, which indicate at which moment a letter may be sent; but on the Delany principle each of the six clerks can peg away as he chooses—he can send at any rate he likes, and he is not disturbed in any way by having any sound to guide or control his ear. The Delany is a very promising system. It may not work to long distances; but the apparatus is promised to be brought over to this country to be exhibited at the Inventors' Exhibition next year, and I can safely say that the Post Office will give every possible facility to try the new invention upon its wires.

One gratifying effect of my visit to the telegraph establishments in America was that, while hitherto we have never hesitated in England to adopt any process or invention that was a distinct advance, whether it came from America or anywhere else, they, on the other hand, have shown a disinclination to adopt anything British; but they have now adopted our Wheatstone automatic system. That system is at work between New Orleans and Chicago, and New York and New Orleans—1,600 miles. It has given them so much satisfaction that they are going to increase it very largely; so that we really have the proud satisfaction of finding a real, true British invention well established on the other side of the Atlantic.

[The result of the ballot was here announced by the President to be as follows:—

President.

C. E. SPAGNOLETTI, M. Inst. C.E.

Vice-Presidents.

Professor D. E. HUGHES, F.R.S.	EDWARD GRAVES.
Sir CHARLES BRIGHT, M. Inst. C.E.	Colonel Sir FRANCIS BOLTON.

Members of Council.

WILLIAM T. ANSELL.	Professor GEO. FORBES, F.R.S.E.
Major R. Y. ARMSTRONG, R.E.	Dr. J. H. HOPKINSON, M.A., F.R.S.
Professor W. E. AYRTON, F.R.S.	Col. E. D. MALCOLM, C.B., R.E.
E. B. BRIGHT, M. Inst. C.E.	ALEXANDER SIEMENS.
R. E. CROMPTON, M. Inst. C.E.	AUGUSTUS STROH.
WILLIAM CROOKES, F.R.S.	JOSEPH W. SWAN.

Associate Members of Council.

Lieut.-Col. A. C. HAMILTON, R.E. | ALEXANDER PELHAM TROTTER.

O. E. WOODHOUSE.

Hon. Treasurer.

EDWARD GRAVES, V.P.

Hon. Secretary.

Colonel Sir FRANCIS BOLTON.

Auditors.

J. WAGSTAFF BLUNDELL. | FREDERICK C. DANVERS.

Honorary Solicitors.

WILSON, BRISTOWS, & CARPMAEL.

A vote of thanks was then passed to the scrutineers for their services.]

Mr. PREECE (continuing): The next branch that I propose to bring to your notice is the question of the telephone.

The telephone has passed through rather an awkward phase in the States. A very determined attempt has been made to upset the Bell patents in that country; and those who visited the Philadelphia Exhibition saw the instruments there exhibited upon

which the advocates of the plaintiff relied. It is said that a very ingenious American, named Drawbaugh, had anticipated all the inventors of every part of the telephone system; that he had invented a receiver before Bell; that he had invented the compressed carbon arrangement before Edison; that he had invented the microphone before our friend Professor Hughes; and that, in fact, he had done everything on the face of the earth to establish the claims set forth. Some of his patents were shown, and I not only had to examine his patents, but I had to go through a great many depositions of the evidence given, and I am bound to confess that a more flimsy case I never saw brought before a court of law. I do not know whether I shall be libellous in expressing my opinion (I will refer to our solicitor before the notes are printed), but I should not hesitate to say that I never saw a more evident conspiracy concocted to try and disturb the position of a well-established patent. However, I have heard that the judgment has been given as the public generally supposed it would be given; because as soon as the case was over the shares of the Bell Company, which were at 150, jumped up to 190, and now the decision is given I am told that they will probably reach 290.

We cannot form a conception on this side of the Atlantic of the extent to which telephones are used on the other side of the Atlantic. It is said sometimes that the progress of the telephone on this side of the water has been checked very much by the restrictions brought to bear upon the telephone by the Government of this country. But whatever restrictions have been instituted by our Government upon the adoption of the telephone, they are not to be compared with the restrictions that the poor unfortunate telephone companies have to struggle against on the other side of the Atlantic. There is not a town that does not mulct them in taxes for every pole they erect, and for every wire they extend through the streets. There is not a State that does not exact from them a tax; and I was assured, and I know as a fact, that in one particular case there was one company—a flourishing company—that was mulcted in 75 per cent. of its receipts before it could possibly pay a dividend. Here we only ask the telephone com-

panies to pay to the poor, impoverished British Government 10 per cent.; and 10 per cent. by the side of 75 per cent. certainly cuts but a very sorry figure. But the truth is, the reason why the telephone is flourishing in America is that it is an absolute necessity there for the proper transaction of business. Where you exist in a sort of Turkish bath at from 90° to 100°, you want to be saved every possible reason for leaving your office to conduct your business; and the telephone comes in as a means whereby you can do so, and can loll back in your armchair, with your legs up in the air, with a cigar in your mouth, with a punkah waving over your head, and a bottle of iced water by your side. By the telephone, under such circumstances, business transactions can be carried on with comfort to yourself and to him with whom your business is transacted. We have not similar conditions here. We are always glad of an excuse to get out of our offices. In America, too, servants and messengers are the exception, a boy is not to be had, whereas in England we get an errand-boy at half a crown a week. That which costs half a crown here costs 12s. to 15s. in America; and, that being so, it is much better to pay the telephone company a sum that will, at less cost, enable your business to be transacted without the engagement of such a boy.

The Americans, again, adopt electrical contrivances for all sorts of domestic purposes. There is not a single house in New York, Chicago, or anywhere else that I went into, that has not in the hall a little instrument [producing one] which, by the turn of a pointer and the pressing of a handle, calls for a messenger, a carriage, a cab, express waggon (that is the fellow who looks after your luggage), a doctor, policeman, fire alarm, or anything else as may be arranged for. The little instrument communicates to a central office not far off, and in two minutes the doctor, or messenger, or whatever it may be, presents himself.

For fire alarms and for all sorts of purposes, domestic telegraphy is part and parcel of the nature of an American, and the result was that when the telephone was brought to him he adopted it with avidity. On this side of the Atlantic domestic telegraphy is at a minimum, and I do not think any one would have a telephone in his house if he could help it.

Mr. E. B. Bright: What is the comparative charge?

Mr. W. H. Preece: When you want a thing you must pay for it. The Americans want the telephone, and they pay for it. In London people grumble very much at having to pay £20 to the Telephone Company for the use of a telephone. I question very much whether £20 a year is quite enough; at any rate, it is not enough if the American charge is taken as a standard. The charge in New York is of two classes—one for a system called the law system, which is applied almost exclusively for the use of lawyers, which is £44 a year; the other being the charge made to the ordinary public, and which will compare with the service rendered in London, which is charged for at £35 a year, against £20 a year in London. The charge in Chicago is £26 a year; in Boston, Philadelphia, and a great many other places it is £25 a year. At Buffalo a mode of charging by results is adopted; everybody pays for each oral message he sends—every time he uses the telephone he pays either four, five, or six cents, according to the number for which he guarantees. Supposing any one of us wanted a telephone at Buffalo, the Company will supply it under a guarantee to pay for a minimum of 500 messages per annum. If 1,000 messages are sent, the charge is less *pro rata*, being six cents, if I remember rightly, for each message under 500, and five cents up to 1,000 messages, four cents per message over 1,000 messages; and so everybody pays for what work he does. It is payment by results. The people like the arrangement, the Company like it because they make it pay, and the system works well. But I am bound to say that, up to the present moment, Buffalo is the only city in the United States where that method has been adopted.

The instruments used in the States are no better—in fact, in many cases they are worse—than the instruments we use on this side of the Atlantic. I have heard telephones in this country speak infinitely better than anything that I have heard on the other side of the Atlantic. But they transact their business in America infinitely better than we do; and there is one great reason for this, which is, that in America the public itself falls into the mode of telephone working with the energy

of the telegraph operator. They assist the telephone people in every way they can; they take disturbances with a humility that would be simply startling to English subscribers; and they help the workers of the system in every way they can. The result is, that all goes off with great smoothness and comfort. But the switch apparatus used in the American central offices is infinitely superior to anything that I have ever seen over here, excepting at Liverpool.

A new system has just been brought out, called the "multiple" system, which has been very lately introduced. I saw it at many places, especially Indianapolis, at Boston, and at New York, where three exchanges were worked by it with a rapidity that perfectly startled me. I took the times of a great many transactions, and found that, from the moment a subscriber called to the moment he was put through, only five seconds elapsed; and I am told that at Milwaukee, where unfortunately I could not go, but where there is a friend of ours in charge, Mr. Charles Haskins, who is one of our members, and he says that he has brought down the rate of working to such a pitch that they are able to arrange that subscribers shall be put through in four seconds.

You will be surprised to learn that there are 986 exchanges at work in the United States. There are 97,423 circuits; there are nearly 90,000 miles of wire used for telephonic purposes; and the number of instruments that have been manufactured amounts to 517,749. Just compare those figures with our little experience on this side of the Atlantic. I have a return showing the number of subscribers in and about New York, comprising the New Jersey division, the Long Island division, New York Staten Island, West Chester, and New York City, and the total amounts to 10,600 subscribers who are put into communication with each other in the neighbourhood of New York alone; and here in England we can only muster 11,000. There are just as many subscribers probably at this moment in New York and its neighbourhood as we have in the whole of the United Kingdom.

I am sorry to delay you so long. I have very few more points to bring before you. I spoke only last week so much about the electric light that I have very little to say on that point. High-tension currents are used for electric lighting in America, and all

wires are carried overhead along the streets. A more hideous contrivance was probably never invented since the world was created than the system of carrying wires overhead through the magnificent streets and cities in America. They spend thousands upon thousands of pounds in beautifying their cities with very fine buildings, and then they disfigure them all by carrying down the pavements the most villainous-looking telegraph posts that ever were constructed. The practice is carried to such an extent, that down Broadway in New York there are no less than six distinct lines of poles; and through the city of New York there are no less than thirty-two separate and distinct companies carrying all their wires through the streets of the city. How the authorities have stood it so long I cannot make out. They object to underground wires—why, one cannot tell. It is something like taking a horse to the pond—you cannot make him drink. So it is with these telephone companies: the public of America and the Town Councils have been trying to force the telephone and telegraph companies to put their wires underground, but they are horses that are led to the pool, and they will not drink. It is said that the Town Council of Philadelphia have issued most stringent orders that on the 1st January next men with axes and tools are to start out and cut down every pole in the city. It is all very well to threaten; but my impression is that any member of Town Council or any individual of Philadelphia who attempts to do such a thing will be lynched by the first telephone subscriber he meets.

This practice of running overhead wires has great disadvantages when the wires are used for electric-lighting purposes as well as for ordinary telephone or telegraph purposes. No doubt the high-tension system can be carried out overhead with economy; but where overhead wires carrying these heavy currents exist in the neighbourhood of telephone circuits, there is every possible liability to accident; and in my short trip I came across seven distinct cases of offices being destroyed by fire, of test-boxes being utterly ruined, of a whole house being gutted, and of various accidents, all clearly traceable to contacts arising from the falling of overhead wires, charged with high-tension current, upon telegraph and

telephone wires below. The danger is so great and damage so serious that, at Philadelphia, Mr. Plush, the electrician to the Telephone Company, has devised this exceedingly pretty cut-out. It is a little electro-magnetic cut-out that breaks the telephone circuit whenever a current passes into the circuit equal to, or more than an ampère. The arrangement works with great ease. It is applied to every telephone circuit simply to protect the telephone system from electric light wires that ought never to be allowed anywhere near a telephone circuit.

Fire alarms are used in America; but in England, also, the fire systems of Edward Bright, Spagnoletti, and Higgins have been introduced, and in that respect we are in very near the same position as our friends on the other side of the Atlantic. Some members present may remember that, when I described my last visit to America, I mentioned how in Chicago the fire alarm was worked by an electric method, and I told you a story then that you did not believe, and which I have told over and over again, but nobody has yet believed me, and I began to think that I must have made a mistake somewhere or other. So I meant, when at Chicago this time, to see whether I had been deceived myself. There was very little room for improvement, because, as I told you before, they had very near reached perfection. This is what they did:—At the corner of the street where a fire alarm box is fixed, a handle is pulled down, and the moment that handle is released a current goes to the fire-station: it sounds a gong to call the attention of the men, it unhitches the harness of the horses, the horses run to their allotted positions at the engine, it whips the clothes off every man who is in bed, it opens a trap at the bottom of the bed, and the men slide down into their positions on the engine. The whole of that operation takes only six seconds. The perfection to which fire alarm business has been brought in the States is one of the most interesting applications of electricity there.

Of course during this visit I waited on Mr. Edison. Many of you know that a difference took place between Mr. Edison and myself, and I must confess that I felt a little anxiety as to

how I should be received on the other side. It is impossible for any man to receive another with greater kindness and attention than Mr. Edison received me. He took me all over his place and showed me everything, and past differences were not referred to. Mr. Edison is doing an enormous amount of work in steadily plodding away at the electric light business. He has solved the question as far as New York is concerned, and as far as central station lighting is concerned; and all we want on this side is to instil more confidence into our capitalists, to try and induce them to unbutton their pockets and give us money to carry out central lighting here.

I met another very distinguished electrician—a man who has hid his light under a bushel—a man whose quiet modesty has kept him very much in the background, but who really has done as much work as anybody on that side of the Atlantic, and few have done more on this—and that is Mr. Edward Weston. He is an Englishman who has established himself in New York. He has been working steadily for years at his laboratory, and works and produces plant with all the skill and exactitude that the electrician or mechanic could desire.

Another large factory I went over was that of the Western Electric Company of Chicago, which is the largest manufactory in the States. That company has three large factories. While I was there the manager, just as a matter of course, handed me over a message which contained an order for 330 arc lamps and for 24 dynamo machines. He was very proud of such an order, but he tried to make me believe that it was an every-day occurrence.

There are no less than 90,000 arc lamps burning in the States every day.

The time has passed very rapidly: I have only just one or two more points to allude to. I think I ought not to conclude without referring to the more immediate things affecting travellers generally and electricians in particular. It is astounding to come across the different experiences narrated by different men who have been on the other side of the Atlantic. One charming companion that we had on board the "Parisian" has

been interviewed, and his remarks appeared in the *Pall Mall Gazette* of Tuesday last, December 9th. There he gave the most pessimist view of life in the United States. He said they were a miserable race—thin, pale faced, haggard, and rushed about as though they were utterly unhappy; and the account our friend gave of what he saw in the United States evidently shows that the heat that did not affect some of us so very much must have produced upon Mr. Capper a most severe bilious attack. Well, his experiences are not mine. Throughout the whole States I received kindnesses and attentions that I can not ~~never~~ forget. I had the pleasure of staying in the houses of most charming people. I found that whenever I ~~you~~ met an educated American gentleman there was no distinction to be drawn between him and an English gentleman. His ways of living, his modes of thought, his amusements, his entertainments, are the same as ours: there is no difference whatever to be found. In Mr. Capper's case I can readily imagine that he spent part ~~most~~ of his time in the halls of hotels, and there you do see those wild fellows rushing about: they convert the hall of the hotel into a mere stock exchange, and look just as uncomfortable as our "stags" who run about Capel Court. You may just as well enter a betting-ring and come away with the impression that the members represent English society, or that that is the most refined manner in which English gentlemen enjoy themselves.

Well, gentlemen, there are just as exceptional peculiarities here as on the other side of the water. The Americans are the most charming people on this earth. When we enter their houses and come to know them, they treat us in a way that cannot be forgotten. I noticed a very great change since I was in America before. Whether it is a greater acquaintance with them or not I cannot say, but there is an absence of that which we can only express by a certain word called "cockiness." It struck me at one time that there was a good deal of cockiness on that side of the Atlantic: that has entirely disappeared. Constant intercourse between the two countries is gradually bringing out a regular unanimity of feeling and the same mode of thought.

But there are some things in which the Americans are a little lax, especially in their history. At one of the exhibitions that I visited, for instance, there was a placard put up—

"The steed called Lightning, say the Fates,
Was tamed in the United States.
'Twas Franklin's hand that caught the horse:
'Twas harnessed by Professor Morse."

Now, considering that Franklin made his discovery in 1752, and the United States were not formed till about thirty years afterwards, it is rather "transmogrifying" history to say the lightning was tamed in the United States.

Again, where the notice about Professor Morse was put, they say that the instrument was invented by Morse in 1846, while alongside it is shown the very slip which sent the message, dated 1844: so that the slip of the original message sent by Morse was sent by his instrument two years before it was invented.

Again, that favourite old instrument of ours which we are so proud of, the hatchment telegraph of Cooke and Wheatstone, invented in 1837, was labelled "Whetstone and Cook, 1840:" so while I am sorry to say they are loose in their history, they are tight in their friendships, and all the visitors receive the warmest possible welcome from them generally, and especially so from every member of our Society belonging to the States.

The PRESIDENT: I am afraid, gentlemen, that at this late hour I can scarcely do anything else than call upon you to give a hearty vote of thanks to Mr. W. H. Preece, in the same spirit in which you have just received him, for giving us a very interesting account of his visit to different parts of America, and especially for all that he has told us about the development, or rather, I may say, the want of progress of our friends across the water during the last few years in connection with telegraphy—for giving us, in fact, such an interesting account of all that is being done there at the present time.

A hearty vote of thanks was given Mr. Preece for his paper.

Mr. W. H. PREECE reminded the members that several exhibits

were on the table, including a pocket sounder that inspectors and linemen carry about with them, insulators used in America, specimens of cable used for telephonic purposes, a key used for telephonic purposes, and a domestic telegraph instrument.

A ballot took place, at which the following were elected :—

Associates:

Nige Macdonald Arnot.	Charles Lever.
Frank King.	Francis Henry Nalder.

The meeting then adjourned until 22nd January, 1885.

THE LIBRARY

ACCESSIONS TO THE LIBRARY FROM AUG. 1 TO DEC. 13, 1884.

BY ALFRED J. FROST, *Librarian.*

(*Works marked with an asterisk* (*) *have been purchased.*)

IT IS PARTICULARLY DESIRABLE THAT MEMBERS SHOULD FORWARD COPIES OF THEIR WORKS TO THE LIBRARY AS SOON AS POSSIBLE AFTER PUBLICATION.

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ORIGINAL COMMUNICATIONS.

EARTH CURRENTS IN INDIA.

By E. O. Walker, Member.

Since commmunicating the paper read on 8th February, 1883, before the Society, I have, by the courtesy of Mr. C. Chambers, F.R.S., Superintendent of the Colaba Observatory, Bombay, had access to his recently-published volume, "Bombay Magnetical and Meteorological Observations, 1879-1882." The tables of observations in this work materially support the correctness of those of earth currents alluded to above. To view the striking similarity between the magnetic and earth-current curves, I must ask a reference to the book placed in the library last December, which contains my observations, reduced to difference of potential in volts. It is evident from the Colaba observations, that while each place has its normal magnetic intensity, which may be said to be its peculiar property, and which does not serve in conjunction with the intensity of another place to produce a current in a telegraph wire joining the two, there is the divisional variation which is timed by the movement of the sun (or of the earth with regard to the sun). These variations are not peculiar to each place, but are conferred upon all places from east to west successively, according to their longitude. Under this influence Madras will receive an access of horizontal force earlier than Bombay, Bellary earlier than Belgaum, Belgaum than Vingorla. There is here, distinctly, the passage of a telegraph wire joining these places through a medium possessing differences of force, from a weak to a strong field. To quote from Mr. Chambers' work, "the typical luni-solar diurnal variation of horizontal force has, it will be seen, the same character all the year round, though the range of it is largest

in the winter quarter, and least in the summer quarter. It has a maximum about three hours before noon, a minimum about three hours after noon, and is nearly nil for four hours before and after midnight." This seems quite to account for the currents observed from east to west before, and from west to east after noon. Plates which sufficiently explain themselves are appended to illustrate these facts. It should be remembered that in those extracted from the Colaba observations, disturbances have been separated from the values given, while in the earth-current observations given in my book, everything actually observed has been plotted. The mean curve would naturally, therefore, deviate from the magnetic curves. I add some observed facts which may be of interest, and plates to illustrate the same. I sought for some time to account for the currents by the differences at the terminal offices of the dirurnal ranges of temperature. I was not successful in obtaining a close correspondence, but plates are given to show what was attempted.

Opportunity was taken at Mahablishwar to observe the effect of elevation. It is 4,500 feet above the sea level. Satara is 30 miles away E.S.E., and 2,300 feet lower. At 10 o'clock a.m. the difference of potential was in favour of Satura ·025 volt; at 2 o'clock p.m. Mahablishwar had the advantage with 0·2 volt. The fact of elevation conferring *apparent* potential is noticed also with Belgaum and Vingorla and Belgaum and Bellary; difference of elevation between the former being 2,473 feet, telegraph line 70 miles long, Belgaum permanent excess of potential ·4 volt; in the latter case, 900 feet difference of potential, a line 200 miles long, Belgaum permanent excess of potential ·6 volt. That is to say, that this permanent excess underlies the diurnal changes. There are stronger currents at the equinoxes as there are greater disturbances of the ordinary magnetic variations.

During perturbations Madras will fall to zero some minutes sooner than Belgaum, showing that the force producing the current is present in the medium surrounding the earth, and will actuate places to the east sooner than others.

The longer lines east and west are affected to a greater degree than short ones (see Plates IX. and XI.), and the same variations

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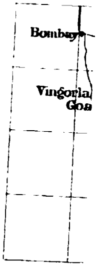

(Between stations inl
but with sea in excess

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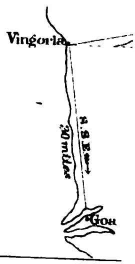
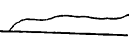


SKETCH TO ILLUSTRATE



affect all lines together, although under the conditions given in the last paragraph.

All the phenomena of earth currents are observed if the earth plates are merely laid on the surface of the ground or simply held in the hand, showing that the internal strata of the earth do not affect the phenomena. North and south lines display very little current, owing doubtless to equality in the increments and decrements of horizontal force in point of time.

Earthquakes and eruptions of volcanoes, as far distant as Sicily and Java, affect earth currents in India, sometimes by intensifying the normal current, sometimes by simply causing abnormal reversals, and, considered together with magnetic and electrical phenomena, appear to be caused by forces exterior to the earth.

As a matter of curiosity, the telegraph line running close at hand, a connection was made with the hot sulphur springs at Aruli, in the Ratuagiri district. Nothing more than the slight and usual current was observed; and taking into consideration the fact that earth plates are buried under such different conditions, some in hard ground, some in deep wells, some in the sea, some in sand, and that nevertheless the same phenomena are common to all lines, there seems no reason to suppose the earth's strata, either by chemical or by thermal processes, are involved in any way in the production of these currents. That they are actuated by a force residing in a medium outside the earth, there seems little doubt.

Belgaum, India,
16th September, 1884.

LETTER REGARDING THE ELECTRIC LIGHTING OF TRAINS.

41, Centre Street,
New York City,
June 9, 1884.

Dear Sir,

I have read with great interest the paper by Mr. Massey on a train-lighting system, in No. 52 of the Journal, and I must regret that I was not present at this meeting, as I should have liked to have recalled to the notice of the members the fact that the first experiment in train lighting was made on October 14, 1881; and also that the Pullman Limited Express, which was lighted throughout with Edison incandescent lamps fed from Faure batteries, commenced running December 1, 1881, and up to the date of my leaving England at the end of April, 1882, had been very successful in its working. The idea was started by Mr. Roberts, of the Pullman Car Company, and by Mr. Knight, the general manager of the London, Brighton, and South Coast Railway; and upon the success of this first experiment it was decided to light the Pullman Limited Express, the cars for which were then in course of construction; hence it will be seen that the first railway carriage lighted by electricity was tried two years and six months prior to the reading of Mr. Massey's paper. Further, as for the system of taking power from one of the axles to drive a dynamo, for feeding secondary batteries, I would here remark that certain experiments with which I had something to do were carried out on the London, Brighton, and South Coast Railway early in 1882, on a train running between Brighton and Kemp Town; and I think I am correct in stating that to Mr. Camille Faure himself we are indebted for the idea of taking the power from an axle of the train for driving the dynamo, and then feeding the secondary batteries from it.

In 1882 I also fitted up a car on the Pennsylvania Railway.

I remain, etc.,
W. LACHLAN.

ABSTRACTS.

J. STEFAN—CALCULATION OF THE COEFFICIENT OF INDUCTION OF COILS.

(Annalen der Physik und Chemie, B. XXII., H. 1, No. 5, 1884, p. 107.)

According to the theory of electro-dynamic induction, the coefficient to be determined is synonymous with the double electro-dynamic potential of the corresponding circuit on itself; properly speaking, with the special value which this double potential assumes when the electro-magnetic unit current flows through the circuit. This potential can be determined by calculation in both cases, since the form of the circuit is determined geometrically, and is relatively simple.

Maxwell's formula for the first B.A. coil is an approximation, and gives the potential of a coil on itself, on the assumption that the cross-section of the annular space occupied by the coil is a rectangle, and that the dimensions of this rectangle are small in comparison with the mean diameter of the coil. The formula consists of two factors, the first being an approximation, and the second a correction which itself is not correct. Lord Rayleigh has also recognised this, and in his experiments has used a different expression.

Leaving aside the author's recalculation of the coefficients for the coils of the B.A. instrument, we may proceed at once to give his own formula.

Let a be the mean radius of the coil, n the number of convolutions, then $P = 4\pi a n^2 Z$ where Z is a pure number. Let Z be expressed by two factors, the first an approximation and the second a correction, so that $Z = Z_1 + Z_2$. If the cross-section of the ring is a rectangle of width b and depth c, then, as Maxwell has shown,

$$Z_1 = \log.\frac{8a}{\sqrt{b^2+c^2}} + \frac{c^2}{6b^2}\log.\frac{\sqrt{b^2+c^2}}{c} + \frac{b^2}{6c^2}\log.\frac{\sqrt{b^2+c^2}}{c}$$
$$- \frac{2c}{3b}\tan.^{-1}\frac{b}{c} - \frac{2b}{3c}\tan.^{-1}\frac{c}{b} + \frac{1}{12}$$

where the logarithms are all to the base e.

Now divide Z_1 into two parts, so that

$$Z_1 = \log.\frac{8a}{\sqrt{b^2+c^2}} - y_1\,;$$

then y_1 is only dependent on the ratio of b to c, and will remain unaltered if these are interchanged.

Putting $\frac{c}{b} = x$,

$$y_1 = \frac{2x}{3}\tan.^{-1}\frac{1}{x} + \frac{2}{3x}\tan.^{-1}x\,.\,\frac{x^2}{6}\log.\frac{\sqrt{1+x^2}}{x} - \frac{1}{6x^2}\log.\sqrt{1+x^2} - \frac{1}{12}$$

As it is only necessary to know the value of y_1 for such values of x as are less than unity, the author gives a table of these values.

The correction term Z_2 consists of a series which goes up with the even powers of the ratio of $\frac{b}{c}$ to a. According to the author's calculation this term is

$$Z_2 = \frac{3\,b^2 + c^2}{96\,a^2}\log.\frac{8\,a}{\sqrt{b^2 + c^2}} - \frac{c^4}{480\,a^2\,b^2}\log.\frac{\sqrt{b^2 + c^2}}{c}$$
$$+ \frac{b^4}{96\,a^2\,c^2}\log.\frac{\sqrt{b^2 + c^2}}{b} - \frac{b^3}{30\,a^2\,c}\tan.^{-1}\frac{c}{b} + \frac{23\,b^2}{640\,a^2} + \frac{221\,c^2}{5760\,a^2}$$

Splitting up Z_2 we have

$$Z_2 = \frac{3\,b^2 + c^2}{96\,a^2}\log.\frac{8\,a}{\sqrt{b^2 + c^2}} + \frac{b^2}{16\,a^2}\cdot y_2$$

where y_2 only depends on the ratio $\frac{c}{b} = x$,
and hence

$$y_2 = \frac{23}{40} + \frac{221\,x^2}{360} - \frac{x^4}{30}\log.\frac{\sqrt{1 + x^2}}{x} + \frac{1}{6x^2}\log.\sqrt{1 + x^2} - \frac{8}{15x}\tan.^{-1} x,$$

for which a table is also given.

Finally, then, the author puts his formula into this shape :

$$P = 4\,\pi\,a\,n^2\left\{\left(1 + \frac{3\,b^2 + c^2}{96\,a^2}\right)\log.\frac{8\,a}{\sqrt{b^2 + c^2}} - y_1 + \frac{b^2}{16a^2}\cdot y^2.\right\}$$

Since the usual forms of earth inductors have two coils, the coefficient of induction will consist of three parts, viz., of the double potential of each coil on itself, and of the double mutual potential of the coils on each other; and the calculation can only be made, in the case where the two coils are close together, by Maxwell's method. If, however, the centres of the section of the two coils are at a distance from each other which is greater than the diagonal of a section, as is usually the case, the calculation may be materially shortened.

The mutual potential, Q, of two coils of equal radius, a, and equal number of convolutions, n, is $Q = 4\,\pi\,a\,n^2\cdot Z$.

If b is the width, c the depth of the rectangular groove of wire, and d the distance apart of the centres of the coils,

$$Z_1 = \log.\frac{8\,a}{d} - 2 + \frac{b^2 - c^2}{12\,d^2} + \frac{2\,b^4 + 2\,c^4 - 5\,b^2\,c^2}{120\,d^4}$$
$$+ \frac{3\,b^6 - 7\,b^4\,c^2 + 7\,b^2\,c^4 - 3\,c^6}{504\,d^6}$$

which gives a first approximation, to which a correction must be added of the form

$$Z_2 = \left(\log.\frac{8\,a}{d} - 2\right)\left(\frac{3\,b^2 + c^2 + 18\,d^2}{96\,a^2} - \frac{15\,d^4}{1024\,a^4}\right)$$
$$+ \frac{7\,b^2 + 23\,c^2 + 60\,d^2}{192\,a^2} - \frac{29\,d^4}{2048\,a^4}$$

The author further considers the correction which has to be introduced for the insulating material and for the spaces between the round wires.

L. BOLTZMANN—DEDUCTION OF STEFAN'S LAW OF THE DEPENDENCE OF RADIATION ON TEMPERATURE FROM THE ELECTRO-MAGNETIC THEORY OF LIGHT

(*Annalen der Physik und Chemie, B. XXII., H. 2, No. 6, 1884, p.* 291.)

Maxwell has deduced from his electro-magnetic theory of light the conclusion that a ray of light or radiant heat when falling perpendicularly on unit surface must cause a pressure on that surface which is equal to the energy contained in the unit volume of ether, in consequence of its vibrations. If this energy is represented by F (t), where t is the absolute temperature, all the heat rays will not fall perpendicularly on the surface. The simplest method is to consider the space in which is the unit volume of ether as a cube with its sides parallel to the three rectangular axes of a system of co-ordinates. As a mean value we may then suppose that one-third of the total energy is propagated parallel to each axis, and hence the pressure on each wall is

$$f(t) = \tfrac{1}{3} \mathrm{F}(t).$$

The same result may also be reached by considering that the rays impinge on the wall under an angle θ; the pressure on the unit surface is then

$$\mathrm{F}(t) \cos.^2 \theta \sin. \theta \, d\theta,$$

and if this differential is integrated between the limits o and $\tfrac{1}{2}\pi$, we obtain also $\tfrac{1}{3}$ F (t).

In a former article on the subject of Bartoli's discovery of a relation between radiant heat and the second law of thermo-dynamics, the author has shown that the following relation holds between the functions F and f, viz.,

$$f = t \int \mathrm{F} \frac{dt}{t^2}$$

of which the differential is

$$t \cdot df - f \cdot dt = \mathrm{F} \, . \, dt;$$

if then, as follows from the electro-magnetic theory of light $f, = \tfrac{1}{3}$ F, we obtain

$$t \cdot d \frac{\mathrm{F}}{3} = 4 \mathrm{F} \, d \cdot \frac{t}{3},$$

and by integration $\mathrm{F} = c\, t^4$, a law which has long ago been empirically established by Stefan. Stefan's law of dependence of the radiant energy on the temperature therefore follows immediately from the electro-magnetic theory of light and the second law of thermo-dynamics.

Conversely from this second law, and from Stefan's law of radiation, follows, that in a space enclosed by athermanous walls of equal temperature the pressure of the radiant energy on the unit of surface is equal to the third part of the energy of radiation contained in the unit volume.

WERNER SIEMENS—AN ARRANGEMENT FOR THE PRACTICAL REPRODUCTION OF THE STANDARD UNIT OF LIGHT.

(*Annalen der Physik und Chemie, B. XXII., H. 2, No. 6, 1884, p.* 304.)

This arrangement is based on the melting, by a galvanic current, of a very thin piece of sheet platinum. The sheet of platinum is enclosed in a metal case, which only allows the light from the glowing platinum to pass through an aperture exactly one-tenth of a square centimètre in area. The sides of the

aperture are conical, and the sheet of platinum is much larger. Hence at the moment of fusion of the platinum a quantity of light equal to one-tenth of the standard unit is emitted through the aperture.

By gradually increasing the strength of the current, it can be arranged that the moment of fusion is reached just when the comparison of the source of experimental light has been very nearly balanced by the standard. From a series of measurements it has been found that the amount of light emitted at the moment of fusion is equal to that of 1·5 English standard sperm candles.

W. VON BEETZ.—STANDARD CELL FOR MEASUREMENT OF ELECTRO-MOTIVE FORCE.

(*Annalen der Physik und Chemie, B. XXII., H. 3, No. 7, 1884, p. 402.*)

There are two essential points in a standard cell which is to be used for absolute measurements of electro-motive force—first, its electro-motive force should be capable of exact definition; secondly, it should remain entirely unchanged when once set up, or should be capable of very easy reproduction. Kittler has shown that the ordinary Daniell cell does not fulfil these conditions. On the contrary, these requirements are reached in a Daniell cell made up with chemically pure zinc and copper, with dilute sulphuric acid and solution of sulphate of copper of a known degree of concentration, and with the two solutions connected by a siphon with pin-point openings, and also filled with dilute sulphuric acid. Using concentrated solution of sulphate of copper, and dilute sulphuric acid of specific gravity 1·075, Kittler found that the electro-motive force of such a cell was 1·195 volt.

Latimer Clark's cell possesses a very constant electro-motive force (1·457 volt) when correctly put together, but it has two drawbacks—the great variation of the electro-motive force with temperature, and the rapid falling-off when short-circuited, even for a short interval of time.

The author has got over this second difficulty by a particular construction which he gives to the cell, whereby the resistance is immensely increased. He filled a U tube, 1 cm. in diameter and with legs 75 cm. long, with a paste made from sulphate of mercury and solution of sulphate of zinc, and then heated so long that on cooling the mass became quite solid and hard. Then the zinc pole was placed in one end of the tube and the mercury pole in the other, and the openings sealed up with paraffin. The internal resistance was measured = 15,700 ohms. If the electro-motive force of the standard Daniell cell described above is correct at 1·195 volts, Beetz's form of Clark's cell has an electro-motive force = 1·442.

The following table shows the falling-off of electro-motive force:—

Cell short-circuited.	E.M.F.
5 minutes	1·440 volts.
1 hour	1·439 "
4 hours	1·439 "
6 "	1·437 "
12 "	1·434 "
48 "	1·408 "

The cell therefore resists for a long time the polarisation, which can but be very small, seeing that the current is about 0·000091 ampère.

The author then had the idea to construct a dry Daniell cell. This he did by mixing fine plaster of Paris with concentrated sulphate of copper solution to a paste, and also concentrated sulphate of zinc. Half a U tube 4 mm. in diameter and 22 cm. long was filled with the one paste, and after it had set the other half was filled with the other paste. A copper and a zinc wire were introduced into the respective legs and the ends of the tube were closed up with paraffin. Three elements of this construction were compared with a wet cell, with the following results (wet cell E. M. F. = 1·000):—

	I.	II.	III.
	0·996	0·993	1·000
	0·998	0·996	0·996
	1·000	0·999	0·993
	—	0·998	0·998
Mean	0·998	0·996	0·997

The cells were then tested for temperature changes:—

II.		III.	
At 0° C.	0·996	at 1° C.	1·007
„ 20°	0·993	„ 21°	1·000
„ 39°	0·983	„ 32°	0·995
		„ 55°	0·981

Hence between 0° and 20° the decrease of electro-motive force is only 0·015 per degree of increase of temperature. The same value for Latimer Clark's cell has been measured by Helmholtz and Kittler, and found to be 0·08.

The falling-off of electro-motive force when short-circuited is shown below:—

I.

Initial value	0·998
After 10 minutes	0·991
„ 35 „	0·988
„ 14½ hours	0·975
„ 15 „	0·986
Open circuit, 5 minutes	0·994

II.

Initial value	1·000
After 1 hour	0·994
„ 15 hours	0·988
„ 20 „	0·988
Open	0·993
After 15½ hours	0·987
24 „	0·986
39 „	0·987
Open circuit, 5 minutes	0·994

III.

Initial value	1·000
After 15 minutes	0·996
„ 50 „	0·994
„ 17 hours	0·989
Open circuit	0·992

In all cases after the circuit had been open for a quarter of an hour the electro-motive force came back to its initial value. The resistance of No. II. was 14,600 ohms, and of No. III. 13,500 ohms.

These dry cells may be very conveniently joined up to form batteries, and are very serviceable for charging quadrant electrometers in place of Zamboni's piles. A battery of 144 elements, occupying only 16 cub. cm., had a difference of potential at the poles of 152 volts.

In a later form of cell the zinc wire was amalgamated at its point, and the remainder coated with shellac; and the concentrated solutions used in working up the plaster of Paris were diluted with one-third part of water.

C. W. ZENGER—UNIVERSAL ELECTROMETER.

(*Beiblätter, B.* 8, *St.* 6, *No.* 6, 1884, *p.* 522.)

In its general form the instrument resembles a Coulomb's torsion balance. The suspended needle, instead of being of glass, wood, or straw, is, however, of steel wire magnetised, at the ends of which are pith or aluminium balls. Projecting radially from the base of the instrument is a graduated arm on which slides a holder carrying a small magnet, over one end of which slides a soft iron keeper. By means of this magnet the sensitiveness of the instrument can be adjusted within wide limits, as the directive magnet exerts more or less influence on the swinging magnet carrying the balls. The charging electrode has also a metal rod fixed to it horizontally, with balls at either end, so that a double action is produced on the suspended balls. The charging-rod ends above the glass case of the instrument in a plate, above which is a second plate attached to a micrometer screw arrangement. These two plates, the upper of which is covered with shellac, can be brought into contact for the measurement of very weak potentials; while for high potentials thin discs of glass coated with shellac can be introduced between the plates, which thus act as a condenser of variable capacity.

A. WASSMUTH—HEAT GENERATED BY MAGNETISATION.

(*Beiblätter, B.* 8, *St.* 8, *No.* 8, 1884, *p.* 665.)

If m is the moment of a magnet, T the absolute temperature, dx the change in the magnetising force, S the specific heat of the iron, then

$$\frac{d\,T}{dx} = -\frac{T}{S}\left(\frac{d\,m}{d\,T}\right)_x,$$

where $\dfrac{d\,m}{d\,T} = \dfrac{C\,m}{x} - B\,m$, C and B being constants.

Combining the equations, and neglecting $d\,T_0$ in comparison with T_0, and putting

$$\int_0^x m\,dx = F_1\,;\ \int_0^x m\,\frac{dx}{x} = F_2,$$

we have

$$\frac{S\,\Delta\,T_0}{T_0} = B\,F_1 - C\,F_2.$$

In this equation, starting from the temperature T_0, the rise in tempera-

ture is given in terms of the change of capacity to be magnetised with change of temperature.

Exact data on the values of the magnetising force x and the moment m have been given by Professor Dorn. He wound a coil of wire of known dimensions and resistance round the glass tubes used by Herwig, and determined the currents induced, with and without iron cores, when a current of about 29 absolute units flowed through the electro-magnet, by means of which the iron core in the glass tube was magnetised. To determine the constant of the galvanometer, the current generated by an earth inductor of known dimensions was passed through it, and the intensity measured by the methods of multiplication and of throw of needle. The magnetising force was determined from the first deflection of the galvanometer caused by the current induced in the spiral wound round the glass tube, on opening and closing the primary circuit when there was no iron core inside. If the iron core was then inserted, the difference in the strengths of the induced currents in the two cases gave a measure of the magnetic moment. From these experiments the author calculated an almost constant magnetising force,

$x = 4{,}076$ mm.$^{-\frac{1}{2}}$, mg.$^{\frac{1}{2}}$, sec.$^{-1}$, and the moment per milligram with a bundle of 19 iron wires as 1,371 mm.$^{\frac{5}{2}}$, mg.$^{\frac{1}{2}}$, sec.$^{-1}$; with 38 wires, on closing circuit 1,701, and on opening 1,532.

To determine the change $\left(\frac{d\,m}{d\,T}\right)$ of the magnetisation with the temperature, the author heated the steel bundles with the magnetising coil in a copper box, and measured the deflection of the mirror of a Siemens galvanometer. From these experiments, he found in the equation $m \cdot \frac{d\,m}{d\,T} = C \cdot \frac{1}{x} - B$, $C = 0{\cdot}1126$, and $B = 0{\cdot}0001296$; and after introduction of a factor of correction $B = 0{\cdot}000116$, and if $x = 4{,}076$ and $m = 1{,}617 \left\{\frac{1}{2}(1{,}701 + 1{,}523)\right\}$, for $d\,m$ the value 20·8. Putting this value into the second equation, we have for 38 wires $\frac{S \,\Delta\, T_0}{T_0} = 221{\cdot}5$, while Herwig found 172·1, when $S = 0{\cdot}105 \times 4{,}155 \times 10^6$; $T_0 = 273$ and $\Delta\, T_0 = 0{\cdot}0001077$ degree, similarly for 19 wires Herwig found 147 and directly 227. The difference in these values may be accounted for by the permanent magnetism, and by the induction currents set up in the mass of the iron itself.

J. G. MACGREGOR—SURFACE RESISTANCE OF AMALGAMATED ZINC IN A SOLUTION OF ZINC SULPHATE.

(Beiblätter, B. 8, St. 9, No. 9, 1884, p. 713.)

A glass trough 20 cm. long, 10 cm. high, and 10 cm. wide was separated in the middle by a glass plate through which passed a very small glass tube. Four very thin amalgamated zinc plates were placed in the trough between strips of glass, parallel to the electrodes, and the trough was filled with pure boiled solution of zinc sulphate. That these plates gave rise to no current was determined by connecting up a galvanometer which gave no deflection.

The resistance of the trough was about 3,000 ohms, and the introduction of the zinc plates could not have altered this value by at most $\frac{1}{15000}$. The resistance at the surface of each zinc plate was then measured by a Wheatstone bridge, in the same way as the resistance of a metallic conductor would be measured, a Thomson dead-beat galvanometer being used, when it was found to be less than 0·0125 ohm.

Dr. E. BÖTTCHER—COMPARATIVE MEASUREMENTS OF THE ELECTRO-MAGNETIC ATTRACTION OF CYLINDRICAL AND BICONICAL CORES IN SOLENOIDS.

(Centralblatt für Elektrotechnik, B. 6, No. 14, 1884, p. 324.)

Mr. Krizik has stated the law: "The force with which the iron bar is attracted into the spiral reaches its maximum when one end of the bar is at the centre of the coil."

This law is incorrect as a generalisation, and only holds good in special cases. The maximum of attraction and the corresponding position of the core in the solenoid are dependent on the ratio of the lengths of these latter to each other. If the core and the solenoid are equal in length, the maximum attraction occurs when the core is for three-quarters of its length within the solenoid. If the core is twice as long as the coil, then the maximum attraction takes place when half the core is in the coil, and half outside. If the core is three times the length of the coil, the point of maximum is when two-thirds of the core are out of the coil. These values hold good equally for cylindrical and biconical cores.

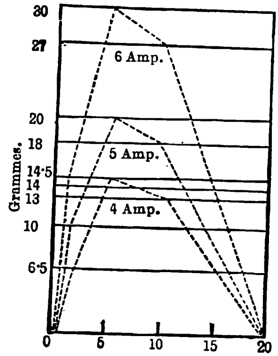
In the author's opinion the biconical core possesses no advantage over the cylindrical core, so that it is indifferent which is used in an arc lamp—at any rate in the case of a single lamp; and it is due to the differential arrangement of coils introduced by Von Hefner Alteneck that a longer core can be used for regulation of the arc.

For the measurement of the attraction of a coil on an iron core in different positions, the author made use of a Salter's spring-balance, to which was hung an iron core 1 cm. in diameter and 20 cm. long. A solenoid, also 20 cm. long, was connected up in circuit with a Schuckert dynamo, and was placed in various positions with respect to the core, both above and below it, but so as always to surround it. The following table gives the results, where the figures are the weight in grammes measuring the attraction:—

Length of Core outside Coil.	Strength of Magnetising Current Ampères.		
	4	5	6
Cm.	Grammes.	Grammes.	Grammes.
15	6·5	10	14
10	13	18	27
5	14·5	20	30
1	6·5	10	14

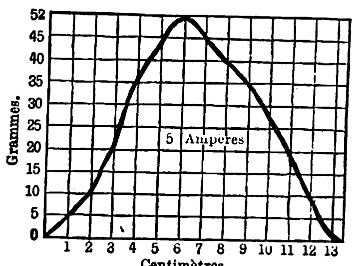
The relation between attraction and length of core is perhaps better shown in the following lines of curves plotted from the above values—the abscissæ represent the length in cm. of the portion of the core projecting beyond the coil, while the ordinates measure the weight in grammes corresponding to the attractive force.

FIG. 1.



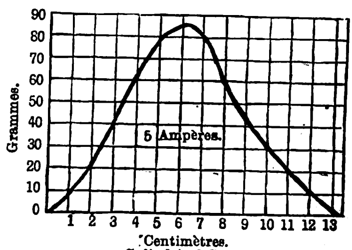
The author has since made some further experiments, from which the curves Figs. 2 and 3 are selected. These show clearly that there is no difference in the behaviour of conical or cylindrical cores under similar conditions.

FIG. 2.



Double Conical Core.

FIG. 3.



Cylindrical Core.

Dr. DIETRICH—THE SHAPE OF CORES IN ARC LAMPS.

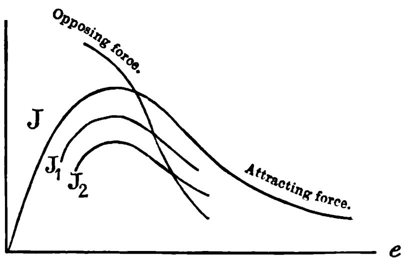
(*Centralblatt für Elektrotechnik, B. 6, No. 19, 1884, p. 485.*)

The results obtained by Dr. Böttcher (see abstract above) give rise to the two following questions:—

1. With reference to good regulation, which form of core is best for single lamps with one coil in the main circuit?

2. What working will be obtained, if in lamps with two coils the cylindrical core is replaced by a biconical one?

In discussing these questions the author considers only those lamps in which there is no releasing arrangement, so that the core has a very long travel as the carbons burn away, and for a single lamp he takes Jaspar's as an example. In this case we have on the one side the attractive force of the coil, and on the other the opposed force made up of the weight of the carbon, carbon-holder, core, etc. Both the attractive force and the opposed force can be considered as functions of the distance between the centre of the core and the centre of the coil, and can be plotted on a system of rectangular co-ordinates. If now a single lamp with one coil in the main circuit is to work properly, it must regulate for the same current at every position of the carbons; for only on this supposition, the outside conditions remaining unaltered, can the length of the arc remain constant from beginning to end of the burning. Hence in every position of the core the attraction corresponding to the normal current must be equal to the opposed force, or, in other words, the curves of the attractive force and of the opposed force must be identical. If they are not, then for *different* positions of



the core equilibrium is only possible for *different* current strengths, as is shown in the accompanying figure where the curves J, J_1, J_2 represent the varying attractive power of the coil for varying currents, while the opposing force is represented by only one curve, since it remains constant. The abscissæ correspond to the distance from centre of core to centre of coil; and equilibrium can only occur at those points where the attraction curve cuts the others. Hence the length of the arc will alter as the carbons burn away and the core descends in the coil.

The author concludes that, in answer to his first question, either a cylindrical or biconical core is equally good.

On consideration of the second question, it is clear that the curve of the attraction of the main coil for the normal current and the curve of the attraction of the shunt coil for constant current in the shunt must also coincide. Now with cylindrical cores these two curves do not coincide, for the curve of the shunt coil forms an asymptote just at that point where the curve of the main coils falls suddenly, and conversely. Accordingly, in order to equalise the two magnetic forces, either a mechanical force must be introduced, as may be

done by means of an excentric wheel, or the cylindrical core must be replaced by one whose curve of attraction falls more quickly; and this is found to be the case with biconical cores.

To sum up, then, for single lamps with one coil only, either form of core may be used; for several lamps in circuit with two coils in each, and no releasing device, the biconical form is preferable.

[N.B.—The term "single" lamp means a lamp used singly on one circuit, as opposed to several lamps arranged either parallel or in lines on a circuit.—*Translator.*]

J. VIOLLE—ABSOLUTE STANDARD OF LIGHT.

(*Comptes Rendus, Vol.* 98, *No.* 17, *p.* 1032, *April* 28, 1884.)

By direct comparison of the absolute unit of light, viz., the light emitted from a surface of one square centimètre of platinum at its point of solidification, with the ordinary Carcel lamp, constructed after the pattern of Dumas and Regnault, the author finds that one Carcel is equal to $\frac{1}{2\cdot08}$ unit; or, taking into account the extent of the illuminating surfaces, the intrinsic intensity of the absolute standard is very nearly eleven times that of the Carcel burner.

Comparisons were also made with Swan lamps worked by a current from thirty Kabath accumulators, the intensity of which could be regulated by a set of resistances in the circuit. Every minute the current (i) and the difference of potential (e) at the lamp terminals were measured, and photometric measurements being made simultaneously, it was easy to deduce the illuminating power of the lamp (E). The value of E for varying values of $i\,e$ was determined both at the beginning and end of each series of observations.

In order to compare the Swan lamp with the standard unit, a Bunsen photometer was used. The vertical rays emitted by the platinum were reflected by a plane mirror placed at an angle of 45°.

The results obtained are tabulated below, where Δ is the distance from the surface of the platinum to the Swan lamp, and D the distance from the platinum to the photometer screen:—

i.	e.	ie.	E.	D.	$\frac{D^2}{(\Delta - D)^2}$	$\frac{ED^2}{(\Delta - D)^2}$
0·88	48·2	42·4	1·64	3,060	4·275	7·011
...	48·5	42·7	1·75	3,033	4·067	7·066
...	48·4	42·6	1·71	3,040	4·108	7·023
					Mean ...	7·023
0·86	47·7	41·0	1·35	3,140	5·177	6·989
...	47·9	41·2	1·37	3,130	5·071	6·947
...	48·0	41·3	1·38	3,130	5·071	6·998
0·89	49·3	43·9	1·70	3,030	4·135	7·029
0·90	49·5	44·6	1·80	2,995	3·857	6·943
					Mean ...	6·986

Deduced from these values the Carcel would be equal to $\frac{1}{2\cdot07}$ unit, which agrees very well with the value found directly, $\frac{1}{2\cdot08}$.

F. LUCAS—APPARENT RESISTANCE OF THE VOLTAIC ARC.

(Comptes Rendus, Vol. 98, No. 17, p. 1040, April 28, 1884.)

The arc acts as a resistance, which is a function of two variables: the strength of the current, and the distance between the carbon points, or

$$R = f(I)\,F(d).$$

The author has carried out a series of experiments with an alternating-current magneto machine, and with carbons 16 mm. in diameter, the current being varied by altering the speed of the machine, and by inserting resistances, from 40 ampères to 75 ampères. In order to study the function f (I), the carbons were made to touch, so that d became zero, and F (d) had a constant value which might be represented by unity. Deducting the resistance of the carbons themselves and of the rest of the circuit, there remained a resistance f (I), which was expressed with a high degree of approximation by the equation

$$f(I) = 0\cdot40 \text{ ohm}\left(1 - \frac{I}{80 \text{ amp.}}\right)$$

Hence the general equation becomes

$$R = 0\cdot40 \text{ ohm}\left(1 - \frac{I}{80 \text{ amp.}}\right)F(d),$$

and this last factor remained to be investigated.

The speed of the machine being maintained constant, the strength of the current varied inversely with the length of the arc, and could be expressed thus:

$$I = 75^{\text{ amp.}}\left(1 - \frac{d}{16\cdot67 \text{ mm.}}\right)$$

where d may have any value from 0 to 9 mm.

From the results the author derives the value

$$F(d) = 1 + \frac{d}{1\cdot4 \text{ mm.}}$$

Hence, finally,

$$R = 0\cdot40^{\text{amp.}}\left(1 - \frac{I}{80 \text{ amp.}}\right)\left(1 + \frac{d}{1\cdot4 \text{ mm.}}\right)$$

In the French lighthouses where the experiments were conducted, the normal current is 50 ampères, and the length of the arc 4 mm.; hence in this case the apparent resistance of the arc would be 0·58 ohm.

H. BECQUEREL.—A NEW METHOD FOR THE DIRECT DETERMINATION OF CURRENT STRENGTH IN ABSOLUTE MEASURE.

(*Comptes Rendus, Vol.* 98, *No.* 20, *p.* 1253, *May* 19, 1884.)

The determination by means of a standard tangent galvanometer involves the exact measurement of H and of the dimensions of the coils of the instrument; while the determination by the electrolysis of a salt of silver gives really the number of coulombs of electricity, and to obtain the value of the current in ampères it is necessary to assume that the current has been constant during the whole time of the experiment.

The author therefore proposes to make use of the rotation produced by a magnetic field on a plane-polarised ray of light as a means of measuring currents.

If we have a bobbin of wire traversed by a current of strength i, and having N complete convolutions, the sum of the forces of the current on a body placed in the axis of the bobbin will be $4 \pi N i$; and if we denote by α the rotation which corresponds for certain lines of the spectrum to a thickness of one centimètre of bisulphide of carbon placed in unit magnetic field, the total rotation will be $4 \pi N i \alpha$.

The apparatus is simple, consisting of a tube of glass one and a half to two mètres long, closed at the end by glass plates and filled with bisulphide of carbon. The bobbin, for which the value of N is exactly known, surrounds the tube, which is further provided at one end with a polariser and at the other with an analyser with graduated circle. The best source of light is the D line of the spectrum, and if a rotation R has been observed, the current is given at once from

$$i = \frac{1}{4 \pi N} \cdot \frac{R}{\alpha}.$$

The value of α was determined several years ago by the author, and found to be $\alpha = 0{\cdot}0463$.

By using a finite length of tube, a portion of the action of the current is neglected, which for each convolution is equal to $1 - \cos. \omega$, where ω is the angle subtending at the end of the tube, the radius of each convolution. If the diameter of the bobbin does not exceed 5 centimètres, the error introduced will not reach one-ten-thousandth.

The above value of α is for zero temperature; as the temperature increases the rotation decreases, but the necessary correction is only one-thousandth for each degree, so that, as the temperature of the bisulphide of carbon can always be known within one degree, no practical error can be introduced from this cause.

The method admits of a very high degree of accuracy, as may be seen from the following example. Suppose a bobbin with 5,000 convolutions, a current of one ampère would rotate the ray of plane-polarised light through about 291′ for the D line. By reversing the current we should have 582′ Now the greatest error in the measurement of the rotation would be 1′, hence an approximation of one-thousandth of an ampère can be attained. It seems, therefore, that this method would be of great value in the calibration of ordinary ammeters.

G. LIPPMANN—A MERCURY GALVANOMETER.

(*Comptes Rendus, Vol.* 98, *No.* 20, *p.* 1256, *May* 19, 1884.)

A mercury manometer is placed between the legs of a fixed permanent magnet, in such a way that the poles of the magnet are on either side of the horizontal part of the manometer tube.

The current to be measured traverses this horizontal part of the mercury in a vertical direction. A difference of level is thus produced in the two legs of the manometer, which is proportional to the strength of the current. The theory of the instrument is this:—The portion of the mercury column traversed by the current represents a movable element of the current, which tends to repel the neighbouring magnet, but as this is fixed it is the current which is repelled in a direction determined by Ampère's rule. The movement of the mercury stops so soon as the repelling force is in equilibrium with the hydrostatic pressure.

Let i be the current strength, and p the hydrostatic pressure, and l the length of the element of the current; the force acting on this element is $H\,l\,i$. The hydrostatic pressure is found by dividing the force by the area of the surface on which it acts. If the element of current has the form of a parallelepiped of length l (as above) and thickness d, this area is $l\,d$. Hence $p = \frac{H\,i}{d}$.

From this formula it follows that the magnetic intensity (H) should be as great as possible, while the thickness (d) of the mercury should be the smallest possible.

In the practical form of the instrument, where a current of one ampère causes a rise of 62 mm., the thickness of the mercury between the poles of the mercury was only one-tenth of a millimètre, and the poles themselves had armatures of soft iron which formed the two sides of the chamber for the mercury.

It is worthy of note that this instrument is reversible—*i.e.*, by exerting a pressure on the mercury a current of electricity can be produced.

J. CARPENTIER—AN EXPERIMENTAL MERCURY GALVANOMETER.

(*Comptes Rendus, Vol.* 98, *No.* 22, *p.* 1376, *June* 2, 1884.)

The author, in a short note, claims priority for a mercury galvanometer depending on the same principle as the one described above. His apparatus was constructed in the month of January, 1881. A flattened glass tube, such as is used for containing phosphorescent powders, was cut to a convenient length, and a glass tube fused into either side, the tubes being bent upwards. Into the open ends of the flattened tube, which was placed vertically between the poles of the magnet, were cemented two plates of platinum, and the whole manometer was filled with mercury. On connecting a battery to the two platinum plates and passing a current, a rise took place in the one tube, which only amounted, however, to 5 or 6 mm., owing to the rather wide separation of the poles of the magnet.

The author proposes to increase the sensibility of the instrument by blowing a bulb on each tube, filling the manometer up to the bulbs with mercury, and above this level with coloured alcohol. He concludes: "My intention is by no means to detract from the merit of M. Lippmann, whose intelligence and character I highly esteem; I only desired to make known the experiments which I have made and the ideas which had directed me."

G. LIPPMANN—A MERCURY ELECTRO-DYNAMOMETER.

(*Comptes Rendus, Vol.* 98, *No.* 25, *p.* 1534, *June* 23, 1884.)

By a slight change the instrument described in the abstract above can be transformed into an electro-dynamometer. All that is necessary is to replace the permanent magnet by a coil of wire, and to cause the current to pass successively through this coil and through the thin parallelopiped of mercury. This instrument measures, of course, the square of the current, and can be used for alternating currents. It can also be constructed so as to serve for absolute measurements. The equation which connects the hydrostatic pressure with the strength of the current is

$$p = \frac{C}{d} i^2,$$

where d is the thickness of the layer of mercury, and C the intensity of the magnetic field produced at the centre of the coil when it is traversed by unit current. C being once determined from the dimensions of the coil, the value of i in absolute measure is found at once from the equation.

In the instrument exhibited to the Académie des Sciences the quotient $\frac{C}{d}$ was equal to 650; hence a current of unit strength (C.G.S.), or 10 ampères, produced a pressure of 650 degrees, or about 650 milligrammes per square centimètre.

In a note, M. Lippmann remarks that the description of M. Carpentier's instrument had never been published, and that he had never heard of it.

G. FOUSSEREAU—CONDUCTIVITY OF DISTILLED WATER AND OF ICE.

(*Comptes Rendus, Vol.* 99, *No.* 2, *p.* 80, *July* 15, 1884.)

In making these experiments the resistance of a column of water was compared with that of a streak of graphite. The specific resistance at 15° C. was found to lie between 118,900 ohms and 712,500 ohms. The great difference may be due to, 1st, the solution of the materials in the glass of which the tubes were made; 2nd, the solution of substances in the air of the laboratory; 3rd, to impurities introduced into the water during its distillation.

From direct experiment it was found that when the water was left in the tube for twenty-four hours the resistance decreased by $\frac{1}{30}$ of its total value. Above 75° C. the resistances vary so rapidly that no measurements were possible. Water heated to 75° C., and then cooled rapidly to 15° C., was found to have

increased four times in conductivity. These variations became, however, very small when the water was kept in stoppered tubes of platinum. The superior limit given above was obtained with water which had been successively distilled in platinum vessels, with potassium permanganate, caustic potash, and lime.

By the addition of one-millionth part of potassium chloride to a sample of distilled water, the resistance was diminished by one-third, and it became five times less by the addition of one-hundred-thousandth. The same effect could probably be produced by half-a-millionth of hydrochloric acid, which is six times as good a conductor as potassium chloride. The wide differences in the results obtained may be readily explained, therefore, by the traces of acids and salts contained in the air of the experiment room.

In studying the influence of heat on the conductivity of distilled water, the author found that the coefficient of change of resistance follows exactly the formula given by Poiseuille for the change in the internal friction of water for changes of temperature, which is of the form $1 + a\,t + b\,t^2$; or, in other words, between 0° C. and 21° C. the resistances of distilled water are proportional to the coefficients of friction.

In experimenting on ice formed from distilled water, the author found that at the moment of freezing the resistance became about 15,000 times greater. The specific resistance varied between 4,865 megohms at — 1° C. and 53,540 at — 17° C. The resistance of ice is affected in the same way as water by the impurities dissolved in it.

A. LEDUC—NEW METHOD FOR THE DIRECT MEASUREMENT OF ABSOLUTE MAGNETIC INTENSITIES.

(*Comptes Rendus, Vol.* 99, *No.* 4, *p.* 186, *July* 28, 1884.)

The apparatus is founded on the mercury galvanometer of M. Lippmann, described above.

A trough one centimètre high and broad, and about one-tenth of a millimètre thick, is filled with mercury, through which a current of 1 to 3 ampères is passed. Into the sides are fixed two vertical tubes 2 to 4 millimètres in diameter, which form the two branches of a manometer.

In the first arrangement adopted one tube was 30 centimètres long, and had attached to it a scale graduated in millimètres; the other tube, 15 centimètres high, terminated in a cup. On placing the instrument between the poles of an electro-magnet a displacement of the mercury takes place.

In a second instrument the longer branch had also a bulb a little above its bend, and the portion above the bulb was filled with water, the height of the shorter branch being reduced to 10 centimètres. If the current in the trough is 6 ampères, and the tube has a diameter of 2 millimètres, one unit (C.G.S.) corresponds to half a millimètre on the graduated scale. The determination of the intensity of the magnetic field is at once deduced from the formula given in M. Lippmann's paper, as soon as the strength of the current and the exact thickness of the mercury are known.

V. BABLON—METHOD OF CONTINUOUSLY CHARGING ACCUMULATORS FROM A SOURCE OF LOWER ELECTRO-MOTIVE FORCE.

(*L'Electricien, Vol* 8, *No.* 81, *August* 15, 1884, *p.* 154.)

The set of accumulator cells are connected directly in series, besides which two wires, one from each electrode of each cell, are connected to two springs. These springs are arranged in a line below two copper or brass rods which are connected respectively to the positive and negative poles of the source of electricity. Below the row of springs is a barrel provided with a series of projecting knobs arranged in a spiral. This barrel is driven by clockwork, and as it rotates it puts the two springs in connection with the electrodes of each cell in contact with rods joined to the poles of the battery. Hence all the cells of the accumulator are charged for a few seconds in succession.

KROUCHKOLL—AMALGAMATION OF PLATINUM, ALUMINIUM, AND IRON.

(*Journal de Physique, Vol.* 3, *March*, 1884, *p.* 139.)

In the course of some experiments, the author plunged into a mercury bath two platinum plates, one of which had been very carefully cleaned by boiling in nitric acid and heating to redness ; the other had not been specially treated. To his astonishment, he found that the cleaned plate had become thoroughly amalgamated. The experiment was repeated with equal success several times, and the amalgamation seems to be due entirely to the state of purity of the surface.

Aluminium plates thoroughly cleaned are not amalgamated on plunging them into mercury; but if the plates are scratched while immersed, and therefore out of contact with the air, small white flakes of the metal are detached and float on the surface of the mercury.

In another series of experiments, the author made use of a voltameter of the following construction :—At the bottom of the vessel some mercury was poured, and covered with a layer of slightly acidulated water, into which dipped an aluminium wire. The mercury and the aluminium wire, without touching each other, are joined up to the negative pole of a battery of two Daniell cells, the positive pole being joined to a platinum plate. On passing the current, hydrogen is evolved on the surfaces of the mercury and aluminium, and effectually frees the two metals from any traces of oxide on their surfaces. On plunging the aluminium wire into the mercury it became amalgamated, but only remained so as long as it was in the voltameter, exposure to the air at once caused oxidation, and small flakes of the metal become detached.

Iron, under the same conditions, behaves in a precisely similar way.

The author concludes, therefore, that the cause which prevents the amalgamation of certain metals is the slight film of oxide which is always more or less present on metallic surfaces which have been in contact with the atmosphere.

MASCART—ELECTRO-CHEMICAL EQUIVALENT OF SILVER.

(*Journal de Physique, Vol. 3, July*, 1884, *p.* 283.)

In a former article of the same periodical (Vol. 1, p. 109) the author had given the value as 11·24 milligrammes for unit current. F. Kohlrausch had found 11·36, and Lord Rayleigh 11·19. Since then, F. Kohlrausch and W. Kohlrausch, working together, arrived at the value 11·183, while Lord Rayleigh's latest determination is 11·18.

The author has therefore been led to verify the corrections introduced into his original calculation, and finds that he has made a slight error. The present article gives the new calculation at length, which leads finally to the value 11·156 milligrammes for unit current.

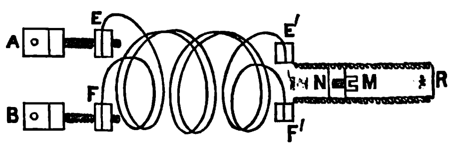
The chemical action of an ampère per second, or of a coulomb, will therefore be—

	Silver deposited. Milligrammes.	Water decomposed. Milligrammes.
According to Kohlrausch	1·1183	0·09325
„ Rayleigh	1·118	0·09323
„ Mascart	1·1156	0·09303

A. GUÉROUT—NEW FORM OF RESISTANCE COILS, BY CARPENTIER.

(*La Lumière Electrique, Vol.* 12, *No.* 18, *p.* 166, *May* 3, 1884.)

The arrangement of these coils is shown very clearly in the accompanying diagram



Two wires are wound on to the core of the coil together, the current, so to speak, going up the coil by one wire and coming down again by the other, the terminals being placed at the base. At the top of each coil is a small spiral of stouter wire, which bridges over the two main wires. Into this spiral screws a metal piece, shown at N, which is partly split, and can be fixed by screwing down M so as to expand N and press it against the main wire. The two main wires having been chosen of such a size and length as will give very nearly the resistance desired, the final calibration is effected by screwing in or out the piece N, so as to lengthen or shorten the path which the current takes through the short adjusting coil. This admits of great accuracy, and moreover affords a ready means of recalibrating the resistance, as there are no soldered joints to be undone and remade.

Dr. H. KRÜSS—COMPARISON OF STANDARD CANDLES.

(*La Lumière Electrique, Vol.* 12, *No.* 18, *p.* 191, *May* 3, 1884.)

The experiments were made at the request of the Committee of the German Society of Gas Engineers.

Three kinds of standard candle were experimented upon—

(1) Munich standard stearine candles, 315 mm. long, 20·5 mm. in diameter at top, 23 mm. at base, mean weight 108·9 grammes.

(2) German standard paraffin candles, 314 mm. lŏng, 20 mm. in diameter, mean weight 83·6 grammes.

(3) English standard spermaceti candles, 252 mm. long, 20 mm. in diameter at top, 22·5 mm. at base, mean weight 75·5 grammes.

It is usual in Germany to fix on certain standard heights for the flame: these are (1) 52 mm., (2) 50 mm., (3) 44·5 mm.

A first series of experiments was undertaken to fix the usual height of the flame, and it was found that this was for (1) from 54 to 56 mm., for (2) from 52 to 54 mm., for (3) from 47 to 48 mm. The average variation was for the stearine and paraffin candles ± 1·98 mm., and for the spermaceti ± 1·57 mm. Hence these latter candles are more constant in the height of their flame. It is, however, very difficult to keep the height of the flame constant without cutting the wick.

The variations in the candle-power, as measured with one of Giroud's photorheometers, is shown in the following table:—

	Flame 44·5 mm. high.	Flame standard height.
Stearine candle	5·6 per cent.	5·4 per cent.
Paraffin "	4·3 "	7·7 "
Spermaceti "	3·0 "	3·0 "

Hence the author concludes that the English spermaceti candle is as accurate as the Carcel lamp, which varies two or three per cent.

As regards the actual quantity of light, the following values were found:—

	Flame 44·95 mm.	Flame standard height.
Stearine	100	100
Paraffin	106·4	97·6
Spermaceti	108·4	85·8

From these values it follows that one Carcel unit is equal to 11·2 spermaceti candles, instead of 9·6, which is the generally-accepted value. It is to be remarked, however, that the height of the flame did not correspond to a consumption of 120 grains of spermaceti per hour, the standard quantity. The author has also settled the melting points thus:—stearine, 53·99° C.; paraffin, 53·75° C.; spermaceti, 43·66° C.

C. T. FRITTS—A NEW FORM OF SELENIUM CELL.

(La Lumière Electrique, Vol. 12, No. 21, p. 311, May 24, 1884.)

The chief point in these cells, the construction of which is not described, is their comparatively low resistance, varying from 500 to 5,000 ohms, combined with their great increase of resistance on passing from light to darkness, the resistance in the dark being twenty or thirty times greater than in the light.

From experiments made with these cells, the author concludes—

1. The resistance varies enormously with the strength of the battery: generally this variation is an inverse one, but occasionally the resistance increases with the electro-motive force.

2. A reversal in the direction of the current may increase the resistance ten or fifteen times, but it returns to its former value when the current is again sent in the original direction.

3. A cell which varied from 10,100 ohms in the dark to 5,700 ohms in the light, with a current from a single Leclanché, showed no variation with a battery of from 12 to 96 bichromate cells.

4. Intermittent currents sometimes increase the resistance, sometimes diminish it, and occasionally are without effect.

5. Slight changes in temperature, 5° or 25°, may cause the resistance to vary enormously.

F. UPPENBORN—KOHLRAUSCH'S INSTRUMENTS FOR MAGNETIC MEASUREMENTS.

(La Lumière Electrique, Vol. 12, Nos. 23 and 25, pp. 368 and 450; Vol. 13, No. 27, p. 9, June 7 and 21, July 5, 1884.)

Mr. Hartmann, of Warzburg, has lately constructed a series of instruments for Professor Kohlrausch, some short notes on which may be interesting, though without drawings it is difficult to give a clear idea of their construction. The first two instruments are magnetometers, which are almost entirely made of wood, so as to avoid the possibility of introducing errors by the presence of even small traces of iron in the metals usually employed. In the first, the magnet is itself the mirror, being very highly polished; in the second, a separate mirror is fixed above the magnet, which in this case is in the form of a bar. An air damper is used to make the instrument dead-beat; and to avoid any electrification which might be brought about by the friction of the air in the damping chamber, consisting of a cylinder of glass, its sides are silvered, with the exception of certain portions which serve as windows.

Either of these two magnetometers can be placed on a stand so as to be at the centre of a large ring of solid copper 400 mm. in diameter and 32 mm.^{2} in cross-section, which can then be used as a tangent galvanometer; or the magnetometer may be replaced by a simple needle on a pivot, for less precise readings.

A small portable galvanometer with a variable damping arrangement consists of an elliptical coil of wire with two separate windings of half an

ohm and thirty ohms respectively. The elliptic ring is 60 mm. high inside and 30 mm. broad, and in this space is hung the polished steel magnet which also serves as mirror. The damper is a cylinder of very pure copper, which can be pushed in so as to enclose entirely the magnet, or it can be withdrawn to any desired degree.

For alternating currents Prof. Kohlrausch has had constructed a unifilar electro-dynamometer: the suspending wire serves as one conductor to the swinging coil, while the second is formed by an electrode immersed in a liquid, which serves at the same time as damper. There are two fixed coils side by side, and the swinging coil to which is attached the mirror is wound on a small ivory frame. With a scale two mètres from the mirror a current of one-thousandth of an ampère produces a deflection of 300 mm. on the scale.

A portable bifilar magnetometer for the determination of the changes in the horizontal component of the earth's magnetism does not differ essentially from the form used at Kew.

Another form of magnetometer has been devised for the study of the magnetic moments of various small magnets. These are carried on metal arms attached to the table of the instrument, so as to assure their being always in the same position relatively to the polished steel magnet which serves as mirror. The scale and telescope are also carried by a longer arm clamped to the stand of the instrument.

The series of articles deals with the methods of measurement and of mounting the instruments, but these particulars would occupy too great a length for an abstract.

G. DUCHÉ—THE ELECTROTECHNICAL INSTITUTE OF LIÉGE.

(*La Lumière Electrique, Vol.* 13, *No.* 32, *p.* 207, *August* 9, 1884.)

This institute, which is due to the munificence of a Belgian senator, M. Montefiore Levi, is under the direction of M. Eric Gerard. The course of study pursued is intended, generally speaking, to amplify the knowledge possessed by engineers leaving the School of Mines, the School of Arts and Manufactures, and the School of Civil Engineering. Examinations are held at the end of the course, which is either of one or two years' duration, and diplomas are granted to those worthy of them. Great stress is laid on the practical side of the studies, and very complete laboratories and workshops have been fitted up, at present temporarily, in buildings belonging to the University of Liége, to which the institute is affiliated. While the theory of electricity is by no means neglected, the pupils are made thoroughly acquainted with its numerous practical applications: telegraphy, telephony, railway signals, electric lighting, transmission of power, electro-metallurgy, all are studied.

G. RICHARD—NORDENFELT'S TORPEDO BOAT.

(La Lumière Electrique, Vol. 13, No. 33, p. 246, August 16, 1884.)

The torpedo, or torpedo boat, of Mr. Nordenfelt is entirely automatic, the screw being driven by force derived from an accumulator in the hold. The steering and manœuvring apparatus are all worked from a fixed point, which may either be on shore or on board a man-of-war, by means of a multiple cable and a set of keys which actuate the circuits of various local batteries on the boat. According to the inventor, with a displacement of 3 tons an accumulator capable of giving out 30 horse-power for a run of about a mile could be easily stowed away in his boat.

The boat floats just level with the surface of the water, so as to be concealed from the enemy's view and protected from his shot. The course of the boat is made apparent to the person directing her, by two tiny masts, one forward and one aft, which can be lowered, if desired, by the agency of an electro-magnet. The rudder is worked by means of two of Currie's electro-magnets through a polarised relay, and the rudder is put over to port or starboard, accordingly as a positive or a negative current is sent through the relay. The screw is of course driven by a dynamo, coupled up to it directly, which can be started and stopped by means of another relay. The torpedo itself, which is carried in the bow of the boat, is fired by the fall of a weight, brought about by the shock when the boat strikes the hostile ship; but this weight is itself held up by a catch which can only be released by a current sent from the directing point.

Prof. BELLATI—ELECTRO-DYNAMOMETER FOR THE MEASUREMENT OF VERY WEAK ALTERNATING CURRENTS.

(La Lumière Electrique, Vol. 13, No. 34, p. 308, August 23, 1884.)

The author has constructed a rough instrument with which he has, however, succeeded very well, by replacing the magnet of a reflecting galvanometer by a small bundle of iron wires 17 mm. long and 0·15 mm. in diameter, which had been very carefully tempered. A very light mirror was attached rigidly to the bundle of wires, and the whole was hung by a bifilar suspension 10 cm. long.

The frame on which the copper wire was wound had a groove 38 mm. long, and the diameter of the ring was 35 mm. The copper wire was 2 mm. in diameter and formed a layer about 8 mm. thick; its resistance was 185 Siemens units, that of the Siemens telephone used in the experiments being 195 units. The deflections were read by means of a telescope and scale. Deflections were obtained on speaking into the telephone, even when this latter was as much as 50 cm. from the mouth.

G. RICHARD—E. WIEDEMANN'S EXPERIMENTS ON THE ELECTRIC DISCHARGE IN RAREFIED GASES.

(*La Lumière Electrique, Vol.* 13, *No.* 35, *p.* 325, *August* 30, 1884.)

In these experiments, which were supplementary to those carried out by R. Wiedemann and Ruhlmann, one of the poles of the electric machine was connected to earth, the other to one of the electrodes of the tube; the other electrode was connected to earth through a galvanometer. Resistances made up of tubes filled with water were inserted either between the galvanometer and the earth or between the tube and the galvanometer. The introduction of resistance has no effect on the strength of the current through the tube, but it increases the rapidity of the discharges. The discharges are always more numerous if the positive electrode of the tube is joined to the machine and the negative to the earth than with the opposite arrangement. Unstratified discharges always take on the stratified form if resistance is introduced into the circuit. Usually the stratifications are more easily produced when the negative electrode is connected to the machine and the positive to earth. The quantity of heat given up to the gas by a given quantity of electricity is about the same, whether the discharge is instantaneous or repeated. Two tubes of different diameters, joined up in series, will give out per unit length the same quantity of heat, whether the number of discharges are increased by inserting resistances or not.

In order to study the question of the influence of the distance between the electrodes on the discharge, the author made use of tubes in which the negative electrode was fixed at the top of the tube, while the positive electrode floated on mercury in the bottom of the tube. If the positive electrode, a wire, is brought near the negative electrode, a plate, the striæ do not change their place, but disappear one after the other, until there only remains a dim reddish light around the anode. The positive light is projected towards the top of the tube until the anode reaches the dark space surrounding the negative electrode, but as soon as the anode enters this dark space the light suddenly falls downwards. On a still nearer approach being made, the discharge ceases to take place from the anode, but is formed between the negative electrode and the mercury. If the positive wire is replaced by a disc, the negative stratifications are deformed, and a thin ring of positive light is formed on the tube itself.

In very highly rarefied tubes the resistance opposed to the discharge increases considerably with the approach of the electrodes to each other, after a certain point. The negative dark space opposes a very great resistance to the positive discharge. The union of the negative and positive electricities occurs about the middle of the tube between the dark space and the striæ. Usually a higher tension is required to bring about a discharge between pointed electrodes than between discs. If the negative electrode be made of chloride or iodide of lead it is quickly disintegrated.

The heat given out by the discharge in different parts was measured by means of a themopile touching the surface of the tube. Starting from the

positive electrode, the heat increases at first slowly, then quickly, and reaches its maximum in the glow; it then decreases, and reaches a minimum in the dark space, which is, however, superior to the value in the positive light. At the kathode itself the heat emitted is very intense. The author does not share the opinion of Crookes as to the negative rays, but thinks that they are rays of light of very short wave length. He is of opinion that these negative rays only play a very insignificant part in the transmission of the current. In fact, the strength of the current does not change if the negative rays are intercepted by a plate of mica. The experiments of Goldstein, confirmed by the author and by Spottiswoode and Moulton, have shown that the vibrations of the negative rays must take place in a certain definite direction with respect to that of the rays themselves. It may be concluded that the vibrations of the positive and negative rays must be in opposite directions, and at right angles to each other. All solid bodies, conductors and non-conductors alike, are entirely opaque to the negative rays.

The author has also proposed a theory to account for the formation of the stratifications. At the positive electrode a polarisation of the dielectric happens, which at the moment of discharge undergoes a change, which passes on and is followed by a current of free electricity. When this dielectric polarisation reaches the negative dark space, where the gases are in an entirely abnormal state, it is reflected, and the returning wave gives rise to interferences, so that the electricity which follows encounters positions of maximum and minimum disturbance. The gases will appear luminous in the former and dark in the latter. The anode itself is always a place of maximum disturbance, and will consequently always be luminous, even when this electrode is brought up quite into the dark space. This luminous layer is very thin; the thicker it is, the wider are the spaces of maximum disturbance.

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